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For Dr Marshall

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AN
INQUIRY
INTO
SOME OF THE EFFECTS
OF THE
VENEREAL POISON
ON
THE HUMAN BODY;
WITH AN
OCCASIONAL APPLICATION OF PHYSIOLOGY,
OBSERVATIONS ON SOME OF THE OPINIONS
OF
Mr. John Hunter and Mr. Benjamin Bell,
AND
PRACTICAL REMARKS.

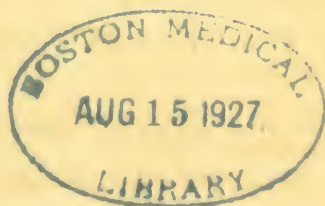
BY S. SAWREY, SURGEON.

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PREFACE.

IF we had no other motive for investigating disease, curiosity alone would be sufficient to invite us. But there are so many opinions upon all diseases which time has discovered, and will yet discover to be erroneous, and there is such a constant desire in the medical practitioner to benefit his patient, that perhaps no subject will be more frequently discussed than the causes and cure of the various maladies which afflict us. Of these, the most prevalent and distressing will be the oftenest considered, and, therefore, the venereal disease has been the subject of much medical reasoning and reflection.

That practice alone is worthy of the name of *rational*, which is governed by physiological truths. If our means of cure be not directed by some knowledge of the operation of causes, and the functions of the body, such practice deserves the name of *empirical*.

A physiological reasoner directing remedies, of the power of which he has already obtained some tolerable notions, can hardly fail to benefit individuals; but if any one administers them at random, he may be right by a lucky chance, but it is more probable that he will do much mischief.

In applying remedies to gonorrhœa, the practitioner must be much governed by his opinions on its *pathology*. Whether he thinks with Mr. Hunter, that the matter of chancre and of gonorrhœa is the same, or with Mr. Bell, that they are caused by two distinct poisons——his practice, if at all consistent, will be much influenced by the opinion he espouses.

When it is clear that the complaint has a venereal cause, we give mercury to cure it:—But if we should be of opinion that its nature is not venereal, we should, of course, omit to use that remedy, and depend upon others. But what will be the practice, and what the embarrassment of a practitioner, desirous of doing what is right, but not having sufficient experience of his own to decide his judgment? He is agitated between those opinions, and cannot strictly follow the mode of cure which either suggests. Doubting whether he has chosen a wrong system, he changes his opinion and practice as often as he per-

perceives an unfavourable variation in the symptoms.

If we examine into the state of the opinions respecting this disease in a more advanced stage, namely, that of lues, the disagreement is as perplexing. The points of contention are not trivial ones, or matters of mere curiosity ; they are of the greatest practical importance. Whether the effects of the venereal poison, when in the blood, be venereal—whether the system has the power of soon getting rid of poison—whether the poison accumulates in the blood, &c. &c. These severally need discussion. The importance of a right decision upon these questions cannot be doubted. According to the ideas we are impressed with upon these points, must our practice be regulated.

These, and such like contradictions and uncertainties, furnished the motive for publishing the following pages, and I hope sufficiently apologize for my adding to the number of works already circulated upon this disease.

In undertaking a work of this nature, it was necessary first of all to decide whether chancre and gonorrhœa were of the same nature. Because, from analogy and a just comparison of their different phænomena, important inferences were to be drawn.

From our imperfect knowledge of the composition of the venereal poison—from the difficulty

and ambiguity of chemical experiments capable of being made upon this matter, it appeared that the safest, easiest, and almost only way to acquire this necessary information, was to see whether these two diseases ever produce each other, or if gonorrhœa ever produces that unequivocal effect of the venereal poison—*lues*. Because, if the poison of each be the same, we might reasonably expect that the one would (sometimes at least) cause the other; or that gonorrhœa would, in some instances, contaminate the system, and produce *lues venerea*.

To acquire this knowledge, two modes offered. First, by the experiments of inoculating with the matter. Secondly, by tracing the disease through the common course of communication. The latter, for many reasons, appeared infinitely more preferable.

To make experiments upon our fellow creatures, must be highly distressing, if we could obtain permission. I do not think the natural solicitude of a practitioner for the welfare of his patient, would permit him to follow up an attempt of this nature to a proper extent. We cannot measure the extent of such experiments! nor appreciate health!

To the individual we can never be justified! Besides, experiments of this nature are not altogether free from error.

In tracing the course, and observing the consequences of these diseases, I have been led to conclude that the poison of gonorrhœa and of chancre is the *same*. The circumstances on which I ground my opinion, occupy the first chapter of this work.

Having gained this information, I thought myself authorized to reason and apply it to the explanation of several of the phænomena attending clap, to answer some objections, and explain some of the important differences between the two effects of the same poison, gonorrhœa and chancre. I then thought it necessary to examine into the nature of the evidences which Mr. Bell brings forward to support a different opinion, and give a few practical hints upon the cure of clap, which the inferences suggest. These comprise a preparatory part.

Proceeding to the subject, namely, a physiological inquiry into some of the singular effects of the venereal poison, it naturally divided itself into two distinct sections of inquiry: First, into the effects of the poison *when externally applied*.—Secondly, into the effects *when circulating in the blood*.

On the effects when externally applied, I have noticed the *continuation* and *extension* of the venereal disease. In these respects it is different from many other maladies. I have attempted a

physiological review of Mr. Hunter's opinion, which, together with some analogical reasoning, has occasioned me to draw an inference different from his, and I have added some observations which seem to strengthen that inference.

In considering Mr. Hunter's theory of the cause of the poisonous quality of venereal matter, some inconsistencies and contradictions struck me. I have endeavoured to investigate how far his opinion is capable of being applied, to explain different incidents of this disease connected with it, and whether it is in conformity to laws of the animal œconomy.

These investigations led me to dispute Mr. Hunter's opinion, to suggest and apply a different one, and make a few practical remarks upon the local treatment of the venereal disease.

Whenever I found necessity to dissent from Mr. Hunter's opinion, I have given my reasons. I have taken the liberty of doing so, conceiving that on all medical subjects, though the most respectful attention should be paid to the suggestions of celebrated men, yet that it is essential to the improvement of the healing art, that it be always considered as open to fair and temperate discussion.

In the last part of this work, I have remarked on the effects of the poison when in the circulating
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ing fluid, and on the principal points in Mr. Hunter's theory of lues venerea. Whether its effects are now venereal—whether the product of venereal sores be infectious—whether the poison continues to circulate in the blood, or is soon thrown out by the powers of the system—whether the poison accumulates—how the product of sores in lues becomes poisonous—whether the effects of the venereal poison be limited—whether the effects produced from the poison, externally applied, be made worse by the blood being contaminated—whether the venereal disease be the immediate cause of others, &c.

This part ends with a few practical admonitions upon the cure of lues; to which is appended, a summary of some of the opinions of Mr. Hunter, and those which I have adopted.

In the execution of this work, which I have found to be difficult, the subjects of which I hope the reader will think with me to be of some importance, I have been under the painful necessity of differing in opinion from two modern and eminent authors. But in the practice of medicine, it is above all things necessary, that there should be no *dictator*. This science, so essentially connected with the happiness of man, for a long time was under the bondage of names, and only began to rise to its
present

present improvements, when its students claimed and exerted the independence of free inquiry.

I know that *innovation* is not always beneficial, and that all *speculation* is dangerous when health is concerned. But if the reasoning of others has not been kept closely connected with physiological truths ; if conclusions have been drawn contrary to just inference, careful comparison, and reasonable analogy ; if theories have been agreed on, which wary experience will not confirm ; if opinions have been recommended, which when applied, divert us from the right and safe practice. In these cases, I trust, it will not only be pardonable to abandon those authorities which we conceive to be erroneous, but it becomes a duty to submit all our contrary ideas to the investigation of others, that the general wisdom of the profession may confirm them if true, or expose them if fallacious.

I publish this book with a hope, that an attempt to explain physiologically this distressing complaint, has some claim to approbation for its intention. Perhaps nothing is more wanting than a just habit of physiological reasoning. Every effort of this sort, therefore, deserves recommendation, even though the execution may not be successful.

There

There will be found in the following pages, a number of typographical and verbal errors, which have accidentally escaped my notice while the work was in the press; I have mentioned some of them in the following errata, for which, and the remainder, I beg leave to apologize.

Bucklersbury,
Jan. 8, 1802.

ERRATA.



- Page 45, line 4, "removed" read "accomplished."
— 52, line 23, "constitutes" read "establishes."
— 53, line 4, "of" read "is."
— 60, line 16; "disease" read "cause."
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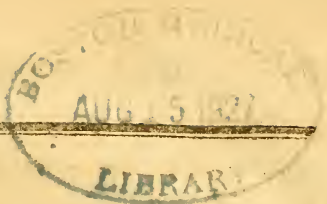
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AN
INQUIRY
INTO THE EFFECTS
OF THE
VENEREAL POISON
ON THE
HUMAN BODY.

PART I.

CHAPTER I.

WHETHER THE SAME POISON PRODUCES GONORRHOEA
AND CHANCER.

THE wildness of human imagination, when it rejects the steady guide of experience, proves to us the necessity of attending to the leader we have deserted. The errors of fancy urge us into labyrinths of difficulties, from which nothing can extricate us, but time, the experience we have undervalued, and a mature deliberation:

To determine upon this question, we must take our faithful guide, experience, along with us—we must not dismiss her after conquering one or two difficulties, with an idea of saving ourselves the trouble which naturally occurs in attending her; we must retain her to the utmost extremity of our journey.

It is surprising that, though the venereal disease has been prevalent for about three centuries, this question should yet require disquisition.

Every inquiry, in which the health of our fellow creatures is implicated, is of great moment. The decision of the present involves the proper treatment of one branch of the disease, and with this treatment human health is most intimately connected.

As I am not attempting a history of the venereal disease, nor the rise of its doctrines, it will be sufficient for me to point out what seems to be the present state of the dispute.

It was for some time supposed, that the two diseases were of the same nature, and proceeded from the same kind of poison: after which, many imagined that they arose from two distinct poisons. Mr. John Hunter favours the first of these opinions,

ons, and intitles his chapter, "Of the poison being the same in gonorrhœa and in chancre." He states, that "it has been supposed by many, that the gonorrhœa and the chancre arise from two distinct poisons;" but he says, "that the matter of gonorrhœa will produce either a gonorrhœa, a chancre, or the lues venerea; and the matter of a chancre will also produce either a gonorrhœa, a chancre, or the lues venerea*."

Mr. Benjamin Bell, Edinburgh, supports the contrary opinion, and brings forward the question thus: "Whether gonorrhœa and lues venerea originate from the same contagion." And he concludes, that "they arise from different *specific contagions*†."

After observing these different opinions, it is necessary, for a moment, to disregard both, to divest the mind as completely as possible of that prejudice to which human nature is so subject—to observe the course of experience, and diligently to seek after truth, which will alone benefit mankind, and stand the test of time.

* Hunter on the Venereal Disease, part i. ch. i. sec. v.

† Bell's Treatise on Lues Venerea, ch. i. Mr. Bell published his Work in 1793.

The contest seems to have originated from a disagreement in the symptoms of gonorrhœa and chancre, and their effects. It has been observed, that they do not equally contaminate the system, and produce lues venerea ; and that the medicine which almost universally cures chancre, has less effect upon gonorrhœa. But these, and such like indisputable facts, we ought maturely to weigh. They point out to us an existing cause for these differences ; but they are not sufficient to warrant a decided conclusion respecting the question. May not these, and other disagreements, depend upon other causes ?

We must inquire whether these differences are *accidental*, owing to the nature of the part affected ; or *permanent*, caused by the properties of the poison.

We must take into consideration, that the seat of the two diseases is different—that the same cause may produce different effects, upon different parts—that the same poison, when mixed with different fluids, may be more or less violent in its operations—and that there may be greater or less attraction of certain fluids to a part, according to its nature and composition. These circumstances require us not to be precipitate in our conclusions, but to extend our researches.

We know little more of the poisons, than their effects upon the human body.

They are in a liquid form, and generally bring on such a state of the parts to which they are applied, that, in the end, a liquid is thrown out by the vessels of the part, which is mixed with the original poison, and partakes of its properties.

When this is accomplished in the urethra, and without a breach of substance in the texture of the part, the effect is called gonorrhœa, and the matter is called gonorrhœal poison.

When an ulcer appears, upon the parts of generation, having certain characterizing marks, we call it a chancre, and affirm the presence of venereal poison. Certain other effects, as particular eruptions upon the skin, ulcerations in the throat and different parts of the body, &c. announce to us a system contaminated with venereal poison; or that this poison has been received into the circulating fluid.

All admit that poison to be venereal, which produces chancre, when externally applied; and when received and circulated with the blood, lues venerea.

After this view of the dispute, this statement of the facts upon which we are to proceed, it would appear, that the true scientific manner of determining this question, is to inquire—if the two diseases ever produce each other—if gonorrhœa ever contaminates the system, and produces lues venerea.—When this information is obtained, we may be allowed to speak decidedly upon the question, and employ our reason to explain the effects; but not before.

It appears to me, that if gonorrhœal matter has clearly and decidedly produced chancre, or contaminated the system in any one instance, the question is determined. It could, in no instance, produce these effects, unless it had the power of doing so. If it has this power, then it resembles the action of the matter of chancre, in its most essential characteristics. It will be universally acknowledged, that a musket-ball has the power of destroying life: yet, in a hundred cases, it may not produce that effect; but we do not, from these, deny the existence of the power. We account for the non-production of the fatal effect, by the concurring accidental circumstances—the direction of the ball—the part it may strike—the intervention of any impediment, &c. I merely ask the same correct reasoning to be extended to the present question. I grant that gonorrhœa very
often,

often, nay, most frequently, does not produce lues venerea; but I know that, in some cases, it has had this unhappy effect. I submit, we must, even in the other cases, admit the existence of the same power; but look to the interposition of other causes, which have suspended the natural action of that power.

That the matter of gonorrhœa is the same with the matter of chancre, has been argued by Mr. Whately, who adduces several respectable opinions in his support*. To arrive at the truth, I have always made particular examinations into the cases which have occurred to me. The result of these examinations proves, that, in general, chancre produces chancre, and gonorrhœa gonorrhœa; but that this is not always the consequence. I will therefore mention a few cases which I have met with, which compel me to be clearly of opinion, that the poison of gonorrhœa may produce gonorrhœa, or chancre; and that it does produce the same effects as that of chancre, when received into the circulating fluid.

CASE I.

1796, Dec. 18th. A GENTLEMAN put himself under my care, for the cure of a clap. Upon most careful examination, not the least appear-

* Mr. Whately on *Gonorrhœa Virulenta*.

ance of chancre was to be perceived. Upon the 30th of the same month, he begged I would call to see a female, whom he was afraid he had injured. Accordingly I did, and upon most attentive inspection, nothing but a clap could be detected, attended with its usual symptoms. Every circumstance which could be collected from the parties, after a careful inquiry, strongly corroborated the assertion, that he was the person who had infected her. These circumstances, of course, I cannot detail; they were, in general terms, the situation and disposition of the patient, and the decided opinion and judgment of the gentleman, together with the time of the appearance of the disease, which was about two days before I saw her: the intercourse had been on the nineteenth or twentieth. At that time, I was much of opinion, that the difference between chancre and gonorrhœa, was owing to a difference in the nature of the poison which caused them. The cure was undertaken by local remedies alone. The symptoms of the disease gave way pretty easily, and entirely disappeared in the course of about two weeks; but the same remedies were continued for some time, to prevent a return. On the 26th of January, 1797, when I was about to take leave of her as being cured, she complained of an eruption which had appeared upon different parts of the body, attended with a considerable itching, and some slight fever. Upon examination, brownish coloured spots, about the size
of

of a small split pea in circumference, were found upon different parts of the body, but particularly the neck breast, and arms. The spots somewhat elevated the skin, and terminated with a lighter coloured margin—the tops of some of the oldest gave a scaly-like appearance, the floating edges of which were turned towards the apices. I was much surprised at these appearances, but could hardly persuade myself that they were venereal: two or three days were allowed to elapse without doing any thing particular. Some circumstances prevented my seeing her for ten days, at the end of which time the spots appeared in greater numbers, attended with a foul spreading ulcer in the right tonsil. No doubt now remaining respecting the complaint, mercury was given according to circumstances, which accomplished a cure.

I am aware that this, and all similar cases may be objected to, by the partizans of a different opinion to what this case points out; but, if strictly examined, I think, this will merit some degree of attention.

If she had no intercourse with any other man, than the gentleman who was under my care, and who had a clap when the last intercourse took place, which I have mentioned to be about the 19th, then the infection which appeared a day or
or

or two before the 30th, came from him. It was he who communicated the disease, the matter of which being received into the circulating blood, produced lues venerea. But should it be doubted that she had no other intercourse, I would then say, whatever complaint any other person had, who tainted her, it produced only a clap, and I would draw the same conclusion; for no other than a clap was at first apparent. If it be still urged, that, perhaps, there was chancre, but so situated that it was not perceptible. I would then answer, it is very improbable, for it is hardly to be supposed that the local remedies had cured it; if not, then some marks of it would have shewn themselves after the cure of the gonorrhœa.

CASE II.

ON the 3d of May, 1796, a well-informed gentleman, from a distant county, applied to me for the cure of a violent clap, which had appeared a few days before. I carefully examined the part (which is my constant practice in all such cases, having seen mistakes arise from the want of this precaution); but not the slightest appearance of chancre, or excoriation, was observed.

On the 16th, he entreated me to take the care of a female, whom he was apprehensive he had injured. The last connection happened about the first of the

the month, about which time his gonorrhœa appeared. He had not the smallest idea, but that if she was injured, he was the cause; being perfectly of opinion, that she had no intercourse with any other man. I am aware that such presumptions as these, are very apt to be fallacious. But, however, there are cases in which, from peculiarity of circumstances, the belief and conviction of a man of sound judgment, who is desired to be wary and discriminating on the subject, must have very great weight. Upon careful examination, I found a chancre on the left side near the orifice of the vagina, with an inflamed, hardened basis, elevated edges, and a brownish white deadened bottom; she had also some pain and enlargement in a gland in the groin on the same side. A clap, mild in its symptoms, accompanied these. Some simple ointment was applied to the sore; mercury was given agreeable to circumstances; and, in about a month, the chancre was well. A slight astringent injection cured the clap in a few days. If the evidences of this case be true, it is an instance of gonorrhœa communicating gonorrhœa and chancre.

C A S E III.

IN November, 1797, a gentleman put himself under my care for the cure of a clap—he had been apprised of its symptoms about three weeks,
and

and had used different injections without any good effect, by the direction of a surgeon—he had not the smallest appearance, or vestige of a chancre, and he assured me he had not had any from the beginning—he had not taken any mercury. There was a swelling in the left groin, about the bigness of half a walnut, extremely tender, and a deep foul ulcer in the tonsil of the right side. Mercury was given so as to affect the system, and an injection used several times a day. The symptoms entirely disappeared in about a month.

CASE IV.

DECEMBER, 1799, a stout healthy gentleman, about twenty years old, put himself under my care for the cure of a clap, which he had felt some days; the cure was attempted by injections,—it proved rather tedious; in about a month, a swelling took place in a gland of the left groin, and a venereal eruption upon different parts of the body. Mercury was now given pretty freely, notwithstanding which, the bubo suppurated, and was opened by the lancet. Mercury was continued according to its effect, and a cure accomplished in about two months.

In these two last cases, there was lues without chancre, and following a gonorrhœa. Of course, the

the gonorrhœa must have produced the lues, unless we suppose a lues without chancre, which is a very rare occurrence. These cases are matters of physical reasoning, and lend powerful support to my opinion, if my inferences are just. I will therefore beg leave to state my argument more fully.

The same person had, from the same infection, a lues without chancre, preceded by gonorrhœa. In this instance, either the lues was produced by the gonorrhœa, as I contend, or he, at the time of connection, received a double infection from another, who was diseased by a chancre and gonorrhœa. Now, if the chancre in the female had produced the lues in the man, which appeared after the gonorrhœa, then that lues so produced, would have been accompanied by a chancre. The chancre is the spot where the poison of the diseased first infects the healthy party, and from which it goes into the system. But in my two last cases, there was no chancre. There is, therefore, no alternative between supposing that the gonorrhœa caused the lues; or, that the female chancre caused it without the intermediate effect of a chancre in the man.

But I appeal to the Profession, if it is not a very rare occurrence indeed, to find lues (where
there

there is no gonorrhœa) without chancre. I submit that it is so rare, that we cannot, in fair reasoning, say, that it has taken place in the frequent instances of gonorrhœa followed by lues without chancre.—Because to make a very uncommon occurrence the cause of a frequent occurrence, is not consistent with any rule of just inference or reasonable analogy.

I could bring forward a number more cases of a similar nature, giving us the like information; but if those which I have related are faithful, they are sufficient for the purpose.

It may not be improper here to confess, that though I began these inquiries, I hope guardedly and without prejudice, yet like most young men, I had allowed myself to form a pre-conceived opinion. I expected to find the two diseases totally different, and never to run into each other. This opinion I became obliged to correct, or else to deny the facts and evidences which I took the greatest pains to collect and scrutinize.

From these facts and reasonings, collectively taken, I feel myself obliged to assert, that the poison of gonorrhœa will either produce gonorrhœa or chancre; or, if received into the circulating blood, it will produce lues venerea. The
poison

poison then of gonorrhœa has the *same* properties, is of the *same* nature as that of chancre, or it could not produce the *same* effects.

I am also equally bound to assert, from general observation, that most frequently the poison of gonorrhœa will communicate gonorrhœa, and a person having a chancre will communicate that disease; and that gonorrhœa does not so soon, nor so often, contaminate the system, and produce lues venerea, as chancre.

I am aware that these facts will not appear so strong to the reader, as to the observer, and especially to the scrupulous supporters of the different opinion; yet, I hope, even these may serve to warrant a probable conjecture, to stimulate to further inquiry, that truth may, at last, be detected, and error banished.

CHAPTER II.

OBJECTIONS ANSWERED, AND THE DIFFERENCE BETWEEN
THE SYMPTOMS AND EFFECTS OF GONORRHOEA
AND CHANCRE EXPLAINED.

PRESUMING upon the validity of my conclusions in the last chapter, I shall proceed in this, to take into consideration some of the most weighty objections to the opinion I have defended, and shall endeavour to account for the difference in the effects.

This is a curious, and somewhat difficult, undertaking. Mr. Hunter has partially attempted it—perhaps I may not clearly understand him; but, as far as I do, I must confess, I think, he has not argued with his usual ingenuity. 1st. It may be asked, if the poison of gonorrhœa and chancre be the same, why does not the former commonly produce ulceration? 2ndly. Why do they not equally contaminate the system? 3dly. Why does not chancre in the same person produce gonorrhœa, and gonorrhœa chancre?

To accommodate or reconcile these differences, which do not seem to depend upon the quality
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of the poison, we must consider the nature and functions of the parts affected.

The inside of the urethra is lined with a tender membrane, which is full of capillary vessels. It may be traced continuous forward, from the lining of the bladder into the skin and cuticle of the glans penis; or from the skin and cuticle of the glans penis, turning in at the orifice of the urethra, running into the inside of the bladder. This membrane possesses new properties unlike that of either the lining of the bladder, or skin and cuticle of the glans penis. It is perforated with a number of holes called *lacunæ*, which are orifices of the excretory ducts, corresponding to, and leading from, a number of mucous glands, dispersed in different parts of the substance of the urethra. These glands, at particular moments, elaborate abundance of mucus, or slimy matter, which may be observed flowing out at the orifice of the urethra. The *lacunæ* have been accurately taken notice of, and delineated by Dr. Cockburn, and others.*

Many of the arteries of this lining appear to be open mouthed, by which a copious fluid is exhaled, to lubricate and defend the canal.

* Haller, Morgagni &c.

The nature of this membrane, the particular degree of irritability in its arteries, give the part appropriate susceptibilities to the influence of certain causes, by whose action it becomes diseased.*

Some of the arteries, I have said, are open mouthed: This circumstance fits, or renders them capable of getting rid of their contents, diseased or healthy, and of throwing them into the canal without any breach in their texture, which otherwise would be a necessary consequence.

Quest. 1. Why does not gonorrhœa commonly produce ulceration in the urethra?

Now what appears to happen upon the application of the venereal poison is this: the poison produces the venereal inflammation in the small arteries of the membrane of the urethra, by which the product of these vessels, which was previously a thin, transparent, watery mucus, is changed, and thrown from their open mouths into the urethra and its lacunæ.

Here I would beg leave to observe, that Mr. John Hunter, in his book upon the venereal disease (p. 29), takes some pains to claim the discovery that the matter of gonorrhœa does not arise from ulceration in the urethra, which he discovered in
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* Dr. Marshall's Lectt.

the year 1753. It is only justice to the departed merit of Dr. Cockburn, to observe, that he asserted the fact, and reasoned upon it, agreeable to the then existing state of knowledge*.—The copy of his book, which I have, is the third edition, which was published in 1718.

In many instances, the venereal inflammation seems to extend to the glands of the urethra; but, as judiciously observed by Mr. Hunter (p. 46), they are not always affected. When these glands become diseased with the venereal inflammation, their healthy product becomes changed, and is forced into the lacunæ, and hence into the urethra.

The discharge is copious, on account of the nature of the part, the healthy actions of its vessels being to secrete and exhale much mucus: on this disease being occasioned by the poison, a proportional quantity of discharge takes place.

Thus, then, ulceration is, in general, prevented by the structure of its arteries. The product occasioned by the disease is thrown out into the urethral canal, by the natural openings in their extremities, and the same purposes are obtained as from ulceration, in a more safe and easy way.

* Cockburn on *Gonorrhœa*, Chap. 4.

When the poison is applied to the dry and dense skin, where the arteries are smaller, and perhaps not so many of them having open extremities, it there also produces the venereal inflammation. The product of the disease is not by the same means got rid of; relief, therefore, not being obtained, the inflammation advances, and ulceration necessarily takes place. These I conceive to be the principal reasons why one is attended with ulceration, and the other not.

Quest. 2d. Why does not gonorrhœa equally contaminate the system as chancre?

In gonorrhœa the discharge is very plentiful: it is not, in general, attended with ulceration; the poison is much more diluted, and with a mixed mucous and puriform fluid. It is deposited in the urethra, and its lacunæ, where little or no pressure is applied, and it finds easy egress out of the canal. These circumstances co-operating, contribute to prevent frequent contamination.

In chancre, there is a breach of substance; the poison is strong, not being much diluted. It is mixed with pus: it remains upon the ulcer, and is subjected to constant pressure and friction. These occurrences make contamination from this effect of the poison frequent. These ideas are
suggested

suggested by Dr. Marshal's conception of the function of the lymphatic system*.

Quest. 3d. Why does not chancre generally, in the same person, produce gonorrhœa, and gonorrhœa, chancre?

These incidents, I believe, are not very unfrequently found to occur. I have known persons having a chancre, which continued for months, become affected after that time, without any further exposure, with a clap. Such instances, I think, can only be fairly accounted for, by admitting that the matter of the chancre had insinuated itself into the urethra, and there produced the disease. Other persons explain the circumstance by the supposition, which has never been proved, that chancre and gonorrhœa were both communicated at the same time, by two different poisons.

Mr. Hunter, I believe, justly observes, that the presence of one disease renders the parts adjacent, not partaking of the disease, less susceptible of the influence of the same, and some other causes, and this has been thought sufficient to account for the question, but it does not appear entirely to clear up the mystery.

* Dr. Marshal's Lectt.

I have often observed fresh chancres caused by the matter of the original one being allowed to remain on the contiguous parts, but more seldom from the matter of gonorrhœa.

It also appears that a person having a chancre, most frequently communicates chancre; and one with gonorrhœa, is more apt to communicate that disease to persons with whom he may have intercourse.

These circumstances might induce us to coincide with those who are of opinion that the matter of gonorrhœa is not real pus, but of a more mucous nature than that of chancre; and who account for the circumstances of gonorrhœa seldom producing chancre, or chancre gonorrhœa, in the same or different persons, upon the principles of chemical attractions—the poison being attracted to the urethra when mixed with mucous matter, and by the dry parts when mixed with pus.

According to the observations of Mr. Bell, gonorrhœa occurs more frequently than chancre;* it might therefore be urged, that the poison originally had a greater attraction to the part which

* Mr. Bell's Treatise, p. 20.

secretes a mucous matter; but that when accidentally united with pus, its affinities are reversed, and it then generally produces chancre. However, at present these observations can hardly be deemed more than speculations, and perhaps ought not to be further urged.

Mr. Hunter is of opinion, that the matter of gonorrhœa is pus;* Mr. Home's experiments tend to the same conclusion.† With due regard to those authorities, I beg leave to observe, I have seldom seen it exactly resemble the pus of a granulating sore, or pus unmixed with other substances.

The respectable authority of Mr. Pott ought not to be omitted, who is of opinion that it is not pus, but widely different from the matter of a "purulent ulcer."‡

I have more than once seen pus in the cavity of the thorax and abdomen, without ulceration; but even in these instances, the appearance was unlike that of the matter flowing from the urethra of those who have clap.

* Mr. Hunter's Treatise, p. 29.

† Mr. Home's Dissertation.

‡ Mr. Pott on Fistula Lachrymalis.

Perhaps we shall not be far from the truth if we look upon the matter of gonorrhœa as a fluid peculiar in itself; not pure mucus, nor yet pus, unmixed with other compounds. From the nature of the part, or the particularity of the disease, it possesses properties which have not yet been precisely detected.

CHAPTER III.

OBSERVATIONS UPON MR. BELL'S DEFENCE OF THE
OPINION HE SUPPORTS;

I CANNOT discard this subject, without making some observations upon the manner in which Mr. Bell defends his opinion upon this important subject. While I acknowledge obligations to this respectable author, for many observations which he has made upon the venereal disease, and surgery in general, I cannot but think that his defence of the opinion, which has been so long suggested (that the matter of gonorrhœa and chancre is different) is lame and ineffective. As the mode of practice in gonorrhœa depends upon a proper decision of this question, every argument brought for and against, ought to be studiously scrutinized; no facts ought to be admitted, but those that have a strict reference to the question.

In the first place, I would beg leave to remark, that Mr. Bell has not put the question clearly and pointedly, "Whether gonorrhœa and lues venerea, originate from the same contagion?" the one, gonorrhœa, is a *local* disease, produced by the poison externally applied to the urethra; and
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the other, lues venerea, an affection produced by a *contaminated* state of the circulating fluid. For want of due attention to this apparent trivial circumstance, unworthy comparisons are made between their different symptoms.

To save trouble to the reader, and that I may not be accused of misrepresentation, I will take the liberty of giving Mr. Bell's own words.—
“ That the symptoms of the two diseases are different, is universally known. A particular detail of such as are peculiar to each, will be given in the ensuing chapters. At present, it is only necessary to observe, that gonorrhœa consists of a discharge of puriform matter from the urethra; which, even by those who support the contrary opinion, is now admitted to be, in almost every instance, a local affection, and that it very rarely contaminates the general habit of body: while lues venerea is a disease of the constitution, arising from the absorption of venereal virus from any part of the surface of the body, but most frequently from the genitals; by which are produced buboes, ulcers in various parts, particularly in the nose and throat, pains and swellings in the bones, with a variety of other symptoms which it is not at present necessary to mention.” Vide p. 3. v. i.

The disagreement here pointed out, would have been as obvious, had the inquiry been established between chancre and lues venerea. It ought to be recollected, that, in general, nay, almost always, lues venerea is preceded by a local effect, from which it arises: lues venerea is seldom the first effect of the poison upon the animal body; it is generally preceded by chancre, which is as truly, and as strictly, a local affection as gonorrhœa. Yet no person doubts a chancre's power of contaminating the blood, and producing lues venerea.

When we institute an inquiry into the similitude of the rise, progress, and effects, of two diseases, we ought to commence with each at its origin; we must not take one in its first stage, and the other in its last. By this we should make imaginary differences, even where there was the most strict relation. In order that a competition be equitable, the contending parties should have equal advantages.

After thus contrasting the two diseases in a manner rather favourable to the opinion he supports, than strictly consonant to nature, Mr. Bell proceeds immediately to determine the question. I will again insert his own words. Page 6th. "In order to support this opinion," (meaning that the

the matter of gonorrhœa and lues venerea is the same in nature) “ data must be received, “ which we know to be inadmissible. We must “ admit, that a person with chancre only, communicates to another, not only every symptom of pox, but of gonorrhœa; and that another with gonorrhœa only gives to all with whom he may have connection, chancres with their various consequences. This ought, indeed, to be a very common occurrence; inso- much, that every practitioner should be able to decide upon it with certainty, if this opinion was well founded: Instead of which, it will be admitted by all, that the one disease being produced by the other is even, in appearance, a very rare occurrence. I have paid much attention to the point in question; and, in almost every instance, a few cases indeed only excepted, and where the most particular inquiries even were made, it has happened, that a person infected with gonorrhœa has received it from another evidently labouring under that disease, and that chancres have been communicated by such as were distressed with chancre only.”

These extravagant inferences, are indeed inadmissible. If the question is to be decided in this manner, any investigation is totally useless.

I hum-

I humbly ask leave to differ entirely from this high authority.—It is not necessary to establish the fact for which I contend, that gonorrhœa should, in every instance, produce “chancres, with their various consequences;” nor that it should, in all cases, produce lues venerea. To prove that the poison of gonorrhœa and chancre is of the same nature in its *essential* or *characteristic* principles, it is only necessary, that, in some instances, gonorrhœa should produce chancre and lues venerea; or, without exciting chancre, contaminate the system. I say, if either of these conditions can be proved to have taken place at one time, it may occur again, and so surely the poison must be the same, or it could not produce the same effects; for we know of no cause which can produce lues venerea, but the venereal poison. Therefore, if we find this effect produced from gonorrhœa, we certainly are authorised to conclude, that it originated, or was caused by that poison.

Mr. Bell takes notice in this paragraph, of what *ought* to be the case, supposing the matter of the two diseases to be the same, without taking into consideration the nature of the parts diseased, or other concurring circumstances. It ought not, in my opinion, to be forgotten, that man was not consulted in the formation of matter and its jaws; he cannot bend facts to his own way of seeing

seeing them, nor agreeable to his own imagination. When we observe, that a cause produces an effect contrary to our expectation (which is almost universally the case), we are not to deny or disregard it. Though gonorrhœa producing lues, be a more rare occurrence than that of chancre, if it ever did so, it is not to be explained away, because of its rarity ; rarity does not make the fact less true. Our prejudices may impel us into error, but facts are immutable and inflexible.

Mr. Bell, also, in this paragraph, admits such facts as oppose, nay, entirely destroy the opinion he endeavours to support ; he excepts a few cases where gonorrhœa had been communicated by persons labouring under chancre, or chancre by such as had gonorrhœa. If these facts have been observed by Mr. Bell, it is surprising that he could support an opinion so incompatible with them.

Mr. Bell's reasons for adopting the opinion are no less surprising ; he gives them in the very next paragraph. I must again give his own words :

“ This, I am convinced, will be very commonly
“ found to be clearly the case ; so that a few
“ instances, bearing some appearance of the con-
“ trary, are much more readily explained on the
“ idea

“idea of the two diseases being produced by different kinds of contagion ; and this may also be said of the few solitary cases that may be met with, of chancre being supposed to terminate in gonorrhœa, and gonorrhœa in chancre, and other symptoms of pox. We can more easily conceive that the same person should, in some instances, receive, and therefore be able to communicate, both kinds of contagion, than that the incidents we are considering should be so seldom met with, were the opinion well founded, of the two diseases being originally of the same nature.”

So then Mr. Bell defends the opinion for the sake of readiness, ease, and accommodation. I again ask Mr. Bell's pardon for thus differing with him. I cannot but think this mode of reasoning highly unphilosophical; he, I think, forsakes the easy and true explanation pointed out by the facts themselves, and adheres to the more hard, difficult, and fallacious one, which he was led into by a mistaken ingenuity.

Mr. Bell then produces, in support of the opinion, several cases of inflammation occasioned by a sudden stoppage of a clap, as he calls it, where, by his good practice, the patients were cured without the use of mercury ; and, in page 14, he deduces conclusions from them. “ It will perhaps

“ perhaps be said, that though this may have hap-
“ pened in a few cases, yet that in others there has
“ been cause to suspect, that lues venerea has been
“ the consequence of a clap disappearing in this
“ manner. In answer to this, it is sufficient for me
“ to shew, that this is, at least a rare occurrence, as
“ I think I am entitled to do, from my never having
“ met with an instance of it. It has been sup-
“ posed, that the sudden check given to the
“ discharge in cases of clap, must necessarily
“ throw the matter into the blood, and that pox
“ must accordingly ensue from it. Were the
“ matter of the two diseases the same, this would
“ happen in every instance; so that, when we can
“ show that it seldom happens even in appearance,
“ we are entitled, from this argument alone, to
“ conclude, that they are produced by two different
“ kinds of contagion; and, where pox has ap-
“ peared at the sudden termination of gonorrhœa,
“ that the two kinds of infection had either been
“ communicated together; or, what may more
“ frequently perhaps be the case, the patient
“ will be found to have received the poxy conta-
“ gion by communication with a diseased woman
“ at the very time he laboured under gonorrhœa.
“ I have already remarked, that lues venerea is
“ frequently produced by absorption while the
“ skin remains entire, and where no chancre or
“ excoriation is perceptible. There is, therefore
“ much

“ much cause to imagine, that in long continued
“ cases of gonorrhœa, many may be infected with
“ lues venerea by communication with others
“ labouring under it; and as this may happen
“ without any external mark of it taking place, it
“ is not surprising that some fallacy should arise
“ from this circumstance.”

Mr. Bell places such confidence in these facts, that because they do not always produce lues venerea, he concludes that the poison of gonorrhœa is not the same as chancre. I shall, therefore, take the trouble of examining these facts, to see if they authorise such conclusions: but previous to this it seems necessary to say a few words respecting gonorrhœa.

Gonorrhœa is a disease in the vessels of the urethra, excited by its cause, which cause is a liquid poison. This disease causes a product to be thrown out at the open mouths of the arteries thus affected, which becomes of the same nature and properties as that which caused it. The product is formed by the disease in the vessels, and deposited from their mouths into the urethra and its lacunæ, from which it finds easy egress out of the orifice of that canal. There is no particular *reservoir*, where a certain quantity of the poison is laid up at the beginning of the disease, which

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quantity

quantity is either to be discharged or conveyed into the system, before a cure can be accomplished, or the disease be removed from its seat. It continues to be formed according to circumstances, until the cause be washed away, or the vessels become unsusceptible to its influence, when the disease vanishes. Be the discharge stopt sooner or later, quickly or slowly, by the interference of nature or art, it is not a necessary consequence that the quantity of this matter, which might otherwise have been discharged, is thrown into the circulating fluid: for, as formed, it is deposited into the urethra and lacunæ, and finds easy egress out of the urethra. This may, perhaps, be sufficient to prepare us for the analysis.

Mr. Bell says, “ I was called, in April 1784,
“ to visit a gentleman who, in a gonorrhœa at-
“ tended with a good deal of inflammation, had
“ been so foolish as to live freely, and to ride much
“ on horseback. This, with the unguarded use of
“ every stimulating injection, put a sudden stop to
“ the discharge ; and at the same time it excited a
“ very considerable degree of pain and inflamma-
“ tion along all the posterior part of the urethra,
“ towards the prostate gland and neck of the blad-
“ der, attended with a painful and frequent desire
“ to make water.

“ On the idea of these being symptoms of pox,
“ he was immediately put under a course of mer-
“ cury ; and, when I first saw him, he had been
“ using it for the space of six weeks. The sur-
“ geon in attendance acknowledges that no ad-
“ vantage had been derived from it ; and the pa-
“ tient himself said that his distresses were daily
“ increasing. They were both, therefore, easily
“ persuaded to lay the mercury aside ; and, by the
“ repeated application of leeches to the perineum,
“ of fomentations, and opiates, to allay the pain,
“ the inflammation soon began to subside ; and,
“ in a short time, he was perfectly well.” P. 111.

It is a matter deserving consideration, what was the real cause of the inflammation in this case, along the posterior part of the urethra. In this particular, I exactly coincide with Mr. Bell, it was the free living, and the use of the stimulating injection, not the venereal poison; and if so, it is impossible the disease could be venereal, and, of course, did not require such remedies, neither could it contaminate the system. Is it reasonable to expect that a stimulating injection could produce a venereal inflammation? a peculiar disease to be occasioned without its cause? No, the effect produced always bears a relation to its cause. We may produce inflammation and a discharge in the urethra, by injecting a strong solution

tion of caustic alkali; but we cannot expect that this disease will be venereal; it will be an effect peculiar to its cause, not venereal.

The strong injection brought on such a state of parts, such an action in the arteries of the urethra, as rendered them *unsusceptible* to the influence or effect of the gonorrhœal poison: no product was formed; no poison assimilated. The gonorrhœa was cured, and another disease was substituted (not caused by the poison) of a totally different nature. And as the poison has no existence until formed and deposited into the urethra and lacunæ, the discharge stopping suddenly is no proof that a certain quantity of that matter is forced into the lymphatics of the part,* and conveyed into the blood.

Supposing that a small quantity of matter was remaining in the urethra, and its lacunæ, at the time when the inflammation commenced about the prostatic part of the urethra, it would not necessarily be forced into the lymphatics of the part; it might be decomposed or washed away by the urine or injection. In short, it would as easily flow out at the orifice of the urethra after the inflammation, as before.

Thus, then, the inflammation, the disease, which was caused by the *injection*, could not be venereal;

* Dr. Marshall's Lectr.

venereal; nor was any particle of the gonorrhœal matter necessarily forced into the lymphatics, and ultimately into the circulating fluid, by the stoppage of the discharge, or the disease caused by the injection. Such like facts then do not support Mr. Bell's opinion.

The second case, which happened in 1788, Mr. Bell does not positively assert to have been produced by the same cause; therefore, I will not waste the reader's time about it. The two last cases he mentions, are so similar to the first, that a separate comment would be useless.

I would then say, that the conclusions deduced from these cases are inadmissible. The facts themselves do not corroborate the opinion, much less are they sufficient to justify a decided inference.

In page 19, Mr. Bell again ingeniously argues, but I fear without proper authority. His own words are—"In the first place, on the supposition
" of the matter of gonorrhœa and lues venerea
" being the same, the latter ought to be a much
" more frequent occurrence than the former, from
" the greater ease with which the matter of infection must, in every instance, be applied to
" those parts on which it can produce chancres

“ than to the urethra, where, instead of chancre
“ or ulceration, it almost always excites gonorrhœa. It is difficult to conceive how the matter
“ by which the disease is communicated should find
“ access in the urethra ; while, on the contrary,
“ all the external parts of the penis, particularly
“ the glands, must be easily and universally exposed to it : and yet gonorrhœa is a much more
“ frequent disease than pox. Cases of gonorrhœa
“ are in proportion to those of chancre and pox,
“ so far as my observation goes, of about three
“ to one ; while it is obvious, that the very reverse should happen, if the two diseases were
“ produced by the same kind of matter.”

However difficult it may appear to Mr. Bell to find access for the matter into the urethra, we must acknowledge the fact. Supposing we were for a moment to admit with Mr. Bell, that the poison of each disease was different, still the access would be as difficult. This supposed difficulty then, is not the smallest objection. The other difficulties have been previously explained, therefore I will not here recapitulate them.

If we preclude ourselves from the advantage of experience, and undertake to explain effects without attending to their causes, and to the nature of the

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the subject or part acted upon, the consequence will be universally dangerous. Upon this, or upon any other subject, we cannot, with any degree of certainty, anticipate effects. If a venereal chancre was shewn to a surgeon, who never had seen or heard of the venereal complaint, is it reasonable to suppose that he could prognosticate its effects upon the system? but is he to deny these effects when they are produced, because they were contrary to what he expected?

Mr. Bell, page 27, takes notice of diseases occasioned from what he calls a *translation* of the matter of clap from one part of the body to another, which he brings forward in support of the opinion he is defending. After attentive consideration, I am persuaded that such cases have not the smallest relation to the circumstances to which they are referred. I will relate one of Mr. Bell's cases, and make some annotations upon it.

“ In the year 1786, a young man applied to
 “ me, with a very troublesome painful disease in
 “ both eyes. The eye-balls were not much in-
 “ flamed outwardly ; but as he experienced an in-
 “ tense degree of pain from the admission of
 “ light, I concluded that the retina, or other deep
 “ seated parts of the eye, were in a state of in-
 “ flammation ; and the membrane of the eye-lids

“ was not only inflamed, but a constant and copious discharge took place from them, of a greenish yellow matter, bearing much the appearance of the matter of a recent clap.

“ The account I received of his disease was this : that he had for eight or ten days laboured under gonorrhœa, the symptoms of which, however, were not more severe than usual ; when, after being heated with drinking port wine, the discharge from the urethra, which had previously been copious, disappeared almost entirely. His eyes, almost immediately thereafter, became painful ; and in less than twenty-four hours, the discharge of matter had taken place from the eye-lids.” P. 27.

I have no conception how a few glasses of wine could instantly *translate* or *remove* the complaint from the urethra to the eyes. But Mr. Bell “ supposed it to have happened through the medium of the circulation.”

From what I have before said, I think it will appear, that if a sudden termination is put to the discharge of gonorrhœa, by cold, astringent injection, or other means, it does not follow that the quantity of matter which might otherwise have flowed from the urethra, is forced into the circulating blood. But supposing, for a moment, that

that we were even to admit this to be the case for arguments sake, how it should be immediately thrown into the vessels of the eyes and produce the effect mentioned, I cannot conceive.

For admitting that a quantity of the matter should be taken up by the lymphatics of the part, mixed with their contents, and conveyed by the long intricate course of these vessels from the part, to the left subclavian vein; it will be poured into the venous blood coming from the left arm, and left side of the head. It is carried into the descending vena cava, mixed with the blood coming from the right side of the head and right arm, and thrown into the right auricle of the heart, where it is mingled with the blood from the ascending vena cava. From the right auricle it will be thrown into the right ventricle; again agitated and propelled into the pulmonary artery, and circulated through the lungs; it will thence be poured with the venous blood (now changed into arterial), by the pulmonary veins, into the left auricle; it will be agitated and thrown into the left ventricle; again agitated and mixed with the blood, and propelled into the aorta. By this time the mixture of gonorrhœal matter and blood will be pretty intimate, and by the aorta and its branches will be dispersed to every part of the body, so that only a very small proportion of the
poison

posion could possibly be circulated into the vessels of the eyes. It seems totally incredible that all this could happen, and a copious discharge be effected from the eyes in twenty-four hours. This incredible quickness must have taken place, if it produced the effect mentioned by Mr. Bell, and through the *medium* of the circulating system.

I suspect Mr. Bell has been here led into an error, from his great anxiety to establish his opinion. I imagine that the poison had taken a much shorter road, and by different agents, I mean the *fingers*, by which the poison had, in this case, been conveyed directly to the eyes, from the seat of the disease.

When the venereal poison is received into the circulating blood, and produces its effects by being applied to the internal surfaces of the arteries, ulceration is the general, if not the universal consequence; and how this happens will be more fully considered in another part of this work.

With respect to the historical facts, approving or disapproving the opinion I have defended, I confess I have not paid a sufficient attention to them to settle my judgment upon the subject on that ground. Mr. Hunter brings them forward to support; Mr. Bell to controvert it.

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I have now finished the observations I purposed, upon some of the principal facts which Mr. Bell presents. I have purposely here omitted discussing several points, those especially which I hope I have somewhat explained in that part of the work where I attempted to account for the disagreement in the effects of gonorrhœa and chancre.

If Mr. Bell's assertion is not well supported by his arguments, the opinion I have defended is of more weight, and deserves more confidence. If my observations upon Mr. Bell's defence, do not show that the facts he has brought forward, when scrutinized, fail to justify the conclusions he has been pleased to deduce, the investigation must terminate in my prejudice. I leave the decision of this, to the impartial and judicious. I have no wish but for candid and unbiassed investigation; truth will then be at last elicited, and when discovered, must prevail.

CHAPTER IV.

PRACTICAL ADMONITIONS UPON THE CURE OF CLAP.

WHILE the subject is fresh in our memories, I wish to make a few practical remarks, which are pointed out by the opinion I have supported; and to apply the information we have laboured to collect, to the mutual advantage of our fellow creatures. This is the highly pleasing part of the subject, and when obtained, the harvest we reap over-pays every trouble.

Two inferences which we have drawn from our inquiries are, that the poison of gonorrhœa and chancre is the same; and that the matter of gonorrhœa does sometimes, though not so frequently, contaminate the system. Most frequently, then, gonorrhœa is a local complaint, not injuring or spreading to any other part of the body.

We know, from experience, that many local complaints are little connected with, or little affect the general system. They may often be cured by remedies applied externally to the disease; but we also know, from the same source, and from the knowledge

knowledge we have acquired of the human body, and its laws, that the cure of local complaints often may be accelerated, or in many instances removed, by remedies acting upon the system at large.

Gonorrhœa is one of these local complaints, which often admit of being removed by local applications. But it appears, that it sometimes after being thus removed, shews itself in the shape of lues venerea. Local remedies then avail but little : this is a bad exchange ; the disease is now formidable, and requires formidable remedies.

I apprehend it will be generally admitted, that a much less quantity of the medicine which cures lues venerea, will, if resorted to in time, prevent its taking place—*ought* we not, therefore, always to attempt prevention.

Three modes of cure, then, naturally present themselves, which are, either to use local remedies, and wait to see if contamination will take place ; or to trust the cure to remedies which act upon the whole arterial system ; or to join the two, and both use external and internal remedies, to cure and prevent.

To demonstrate which of these plans will be productive of the greatest general good, I confess I am not sufficiently prepared. It would be necessary, before the proof can be given, to know the exact proportion of cases in which contamination would take place without the use of mercury, and the individual disadvantage resulting from this affection. One man's practice will not admit of the smallest criterion; the cases of a number of years, and of many practitioners, ought to be collected. Information still more difficult must also be obtained; I mean the exact physical injury done to the constitution by such a quantity of mercury as will prevent contamination. This information, I say, must first be collected, before the decision can be attempted in this way; therefore, I recommend the investigation to persons with more opportunities and better abilities than my own.

But for the safety, advantage, and security of individuals, I think we *ought* justly to prefer the last mentioned plan, and take the advantage of a speedy and preventative cure. This practice, I think, ought to be adopted, for various reasons. First, I think I have seen more advantage from giving small doses of mercury during the cure of clap, than has been balanced by the pernicious effects of that remedy so employed; while, at the
same

same time, this quantity I have found to operate as a prevention to contamination. Secondly, in my own private practice, I have met with a number of instances where the matter of gonorrhœa was productive of lues venerea. Thirdly, because I am persuaded, a small quantity will prevent clap contaminating the blood: and a great deal is often necessary to cure that disease, which in many constitutions does infinite harm.

The local remedies which are used in clap, in the form of injection, are various. Such are to be preferred, which, from experience, are known most often to relieve without injuring the part.

They require to be used with a great deal of caution; the nature of the part, and the diseases to which it is subject, ought to be taken into mature consideration before the use of these remedies. We should be careful not to substitute one disease for another—our local remedies should be managed agreeable to the tenderness of the organ, and the constitution of the patient. I believe we have no certain criterion to judge of what will certainly obtain the desired end in each particular person. We must, therefore, carefully feel our way, and by actual experience come to such remedies, and their respective strength, as may be used with advantage and impunity.

It appears also of great consequence, accurately to discover the *precise* seat of the disease. No good can result from applying our remedies to a sound part of the urethra; but much mischief, I am convinced, may ensue. The urethra is a tender part, and easily becomes diseased from the sound state, by the use of sharp applications. For want of due attention to these circumstances, I have found *strictures* formed further down the urethra than the disease reached.

With respect to the quantity of mercury to be given in cases of clap, to hinder or prevent lues venerea, small doses in general, according to the constitution of the patient, appear to be sufficient. Such quantities as sensibly affect the pulse, without exciting much effect upon the mouth—such quantities I have universally found sufficient, and I am much deceived if this effect upon the vascular system will not assist in curing the local disease.

END OF PART I.

PART



PART II.

CHAPTER I.

THE EFFECTS OF THE VENEREAL POISON UPON THE
HUMAN BODY WHEN EXTERNALLY APPLIED.

IN the first chapter of the first part of this inquiry into the effects of the venereal poison, I have hazarded to infer, from a few cases given out of a number of a similar nature, that the matter of gonorrhœa does sometimes produce chancre and lues venerea: it therefore may be called venereal poison; it is of the same nature in its characterizing principles.—Presuming upon the truth of these inferences, I have attempted, in the second chapter, to explain, physiologically and chemically, some of the most obvious objections to the opinion, and differences between the effects of gonorrhœa and chancre: and have ventured to adduce, that they result from the nature of the

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part affected, and a difference between some of the component parts, uniting with the same poison. In the third chapter, I was compelled, from the nature, connection, and practical import of the subject, to examine some of Mr. Bell's most argumentative objections to the opinion I have defended, and have dared to conclude, that, though ingenious, when justly reasoned upon, they fail to give the expected support. In the fourth and last chapter, I have given a few practical hints, which seem to be suggested by the inferences and reasonings deduced, and which lend some support to the opinion I have vindicated.

It will now be perceived, that the first part of this work is rather a *preparatory* one to the future designs of the author; (which ought, therefore, to be kept in mind through the ensuing ones) preparing him to take into consideration the *philosophy* of the venereal disease.

This inquiry naturally divides itself into two sections.—First, the effects of the venereal poison when outwardly applied.—Secondly, its effects when circulated in the blood. The first of these inquiries, of course, is the subject of this part, which I shall enter upon without any farther preliminary.

The

The venereal disease is most frequently communicated by intercourse of the sexes, one of which being diseased, infects the other. During the connexion, the sound parts of the one are exposed to the diseased parts of the other—the poison being, by this means, brought into contact with the sound and healthy parts, exhibits itself by various effects, according to the nature of the part diseased.

If brought in contact with the external surfaces of capillary arteries, which are large and have open extremities, as in the urethra, it varies the healthy muscular action in these vessels; this variation occasions a copious discharge to be elaborated from the blood or fluid which they circulate. The product is thrown out at the open extremities of these vessels, into the urethra and its lacunæ. This product possesses similar poisonous properties as that first applied.

It also effects a variation in the healthy action of the small arteries of the dense dry skin and cuticle, the arteries of which smaller, and not so many, perhaps, terminate in open extremities, but run into veins by continuity of canal. This variation produced by the poison, now constitutes what may be called (tho' without precision) venereal inflammation, which runs into suppuration

and ulceration. This product, or suppuration, is also venereal.

If it excites inflammation, which does not run into suppuration, but ends in *resolution*, the disease terminates ; unless the matter be conveyed to some other part of the body, where it again produces its effects.

The first effect taken notice of, is called *gonorrhœa*. It is an *accidental* circumstance attending this effect of the poison, that it terminates without the interference of art. How this *casualty* happens, will be hereafter considered.

The second effect is called *chancre*. Without the operation of medicine, it does not cease, at least extremely seldom. Without the assistance of art the effect continues, the disease spreads, and, by its encroachments, new parts are attacked.

The venereal poison, I presume, cannot be doubted, is the cause of the disease (which has been called the venereal inflammation) in the vessels of the part, where it immediately produces, and constitutes the disease : and venereal poison is found to be the product of that venereal inflammation.

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I submit that the poison accidentally applied to the part, is the *cause*; that action or effect brought on in the irritable fibre of the small arteries of the *disease*: the redness, heat, pain, ulceration, &c. consequences or effects of the disease, which may, in medical language, be called *symptoms*. Should this be objected to, I would ask what conditions do really constitute these three *essential* distinctions?

If we constantly observe from the application of a certain kind of matter, denominated venereal poison, that a peculiar effect be produced; is it not scientific and useful to infer, *that*, to be the cause? And that effect, that state of parts which we have constantly observed to follow, and never to precede, to be the effect of the operation of that cause? And should it not be termed the disease? Yet, I submit, that this state of parts which is produced by the cause, is the actual disease. If it be not, what is the disease? The disease does not exist in the mere name we may habitually give to it. It has an actual existence. Names are but convenient sounds used in language for the sake of communicating our sensations, or ideas. To a certain affection of the human frame, we give the name of fever. This term fever, explains nothing of itself. It is a mere sound, applied by the consent of society, when a certain malady affects an individual.

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vidual. There is no disease in the adopted term; the disease consists in that affection of the frame brought on by its cause, of which fever is merely the verbal index. And ought not these effects of the disease, in medical language, to be called symptoms, which immediately proceed from the injured function, or healthy action of the part, and which succeed the disease, and disappear when that disease is removed?

We will wave entirely, for a moment, the consideration of the accidental occurrence mentioned, which is the termination of that effect of the venereal poison, called clap: and, first of all, take into consideration the cause, or causes of the continuation and spreading of the disease, when ulceration or chancre is the consequence.

CHAPTER II.

A REVIEW OF MR. HUNTER'S THEORY ON THE CONTINUATION OF THE VENEREAL DISEASE.

WHEN the venereal poison produces ulceration, or chancre, the disease appears to have no tendency to stop; on the contrary, it continues, and extends to the sound adjacent parts. We should not suspect that this effect could pass unobserved by the philosophic mind of Mr. Hunter; nor indeed did it. He observed this phenomenon, and his ingenuity soon found an explanation. He states that this effect of the disease was accomplished by its *specific* power, and upheld itself independently. As this explanation appears to me to be inconsistent with the principles of action of muscular parts, I presume to make a few general remarks upon it.—But, first, I beg leave to insert his own words:—

Part II. sect. 3d, “ When a secreting surface
 “ has once received the inflammatory action, its
 “ secretions are increased, and visibly altered.
 “ Also when the irritation has produced inflam-
 “ mation and an ulcer in the solid parts, a secre-
 “ tion of matter takes place, the intention of
 “ which in both seems to be to wash away the

“ irritating matter: so that it is the end of irri-
“ tations to produce their own destruction, like
“ a mote in the eye which by increasing the se-
“ cretion of tears is itself washed away. But in
“ inflammations arising from specific or morbid
“ poisons, this effect cannot be produced; for
“ although the first irritating matter be washed
“ away, yet the new matter formed has the same
“ quality with the original; and therefore upon
“ the same principle, it would produce a perpet-
“ ual succession of irritations, and of course se-
“ cretions; even if there were no other cause for
“ the continuance than its own matter. But the
“ venereal inflammation is not kept up by the pus
“ which is formed; but like many other specific
“ diseases, by the specific quality of the inflam-
“ mation itself. This inflammation, however, it
“ would appear, can only last a limited time;
“ the symptoms peculiar to it vanishing of them-
“ selves by the parts becoming less and less
“ susceptible of irritation. This circumstance
“ is not peculiar to this particular form of the
“ venereal disease; it is perhaps common to
“ almost every disease that can affect the human
“ body. From hence it will appear, that the
“ consequent venereal matter has no power of
“ continuing the original irritation; and indeed
“ if this were not the case there would be no
“ end to the disease.”

Mr.

Mr. Hunter, and others, I believe, have been accused of giving the influence of mind to parts of the body as producing several observable effects. By the word intention, in the quotation, I conceive is meant the final intention of nature, not intention in the part irritated ; and it is truly observed, that causes often produce such effects on irritable parts as to throw off the noxious substance.

In the instance mentioned, a mote irritates the vessels of the eye, a copious secretion of tears is the consequence, which often wash away the extraneous matter.

If any foreign matter is forced into the flesh, inflammation and a formation of matter may be the consequences—the matter often tends outwards, and an external opening is thereby made ; the foreign substance is thrown out, and the disease disappears. But whether it be consistent with laws of the animal œconomy, that an effect, when once produced, should continue for ever, or for a long time, of its own accord, or independently of its primary, or any other cause, I wish to consider.

When the different organs of the body are in an healthy state, and their causes of healthy action presented, they move or act in an healthy manner. If those causes be withdrawn, the parts, though
in

in a state capable of action, remains at rest. I presume these observations will be admitted, therefore it is hardly necessary to elucidate them, by saying that the vessels of the liver, which secrete bile, and those of the kidneys, which form urine, not only cease to perform those secretions, but remain at rest, when the course of the blood (which answers the double purpose of stimulating the vessels into action and supplying the ingredients for the new compound) is stopped.*

The reasons given for the continuation of the venereal inflammation are two fold: First, because “the new matter formed has the same quality with the original; and therefore upon the same principle, it would produce a perpetual succession of irritations;” but he attributes the effect to another cause, which he calls the “specific quality of the inflammation itself.” This, however, he finds not to be sufficient, for he says, the inflammation in gonorrhœa “can only last a limited time.”

Though gonorrhœa often ceases without the aid of medicine, yet when the effect of the poison is chancre, this kind of termination does not happen, which Mr. Hunter allows, for in page 35, he observes, “The circumstance of the disease ceasing spontaneously, only happens when it attacks a secreting surface, and when a secretion

* Dr. Cullen's Lectt. Dr. Marshal's Lectt.

“ of pus is produced ; for when it attacks a non-
“ secreting surface, and produces its effects there,
“ that is an ulcer ; the parts so affected are ca-
“ pable of continuing the disease, or this mode
“ of action for ever, as will be taken notice of
“ when we shall hereafter consider chancre.”

It may then now be observed, that according to Mr. Hunter, the venereal disease “ is kept
“ up by the specific quality of the inflammation
“ itself,” but without attempting to explain what Mr. Hunter means by his undefined “ specific quality of the inflammation” (which he himself, perhaps, would have found some difficulty in doing). I shall understand him, that the venereal poison has produced an action or effect in the small arteries of the part which continues and supports itself without the aid of any other power ; or, in short, the disease continues itself. Does not Mr. Hunter, in this instance, account for an effect, without the assistance of a cause ? And does not this appear inconsistent with every law, physical or metaphysical, yet discovered ?

How far this account is consonant with the operation of causes and effects, and to the evidence of experience in this disease ought maturely to be weighed.

In no instance does the human body become diseased *without a cause*. Take away that cause and its physical injury, and the effect will cease : or if, by custom, or any other means, the part becomes unsusceptible to the influence of the cause, the disease equally terminates.

It is true that some apparent objections are presented against these *positions*, for it may be said, we find inflammation continue after the primary cause has been removed.—If an extraneous body fall into the eye, which produces inflammation, that inflammation may continue some days subsequent to its removal. But this is by no means an instance of the continuation of a disease without the aid of a cause ; though the primary disease indeed has been removed, yet another cause has been substituted. The inflammation does not continue from any specific influence inherent in itself ; nor from any disposition or capricious state of the mind in the part diseased. The physical injury done to the vessels by the extraneous substance, now remains the operating cause, and by its effect, the inflammation necessarily continues.

If such a degree of heat be brought in contact to a part of the human body, which is sufficient to change the healthy muscular action in the small arteries, so as to constitute as light degree of inflammation,

mation, that heat may strictly be called the cause, and the varied degree of muscular action in the small arteries, the disease. Take away that heat, and the effect will almost immediately terminate. But if the heat has been so powerful as to injure the physical structure of the vessels, the disease may continue for days; that injury, it would appear, now becomes the cause of the continuation, which will continue to operate until the injury be repaired in the vessels, or the susceptibility vanish, when the disease disappears.

If inflammation be excited in any part of the body from the effect of a cause, and it extend to the neighbouring parts, this extension, I would submit, must also have a cause. I cannot conceive it an act in the disease itself to seize upon the sound healthy adjacent parts of its own accord, without the help of a co-operating cause. The structure or susceptibility of the collateral vessels, I am of opinion, has been so altered, that the blood which previously produced the healthy action in them, now occasions the inflammatory one, and thus the disease spreads.

I am aware of the difficulty of proving these general observations; they are on a subject on which experiments which would satisfy a medical in-

inquirer, are difficult to be made ; we must rather content ourselves with reasoning from physiology, and careful analogy. It is also obvious, that a variety of objections may be brought against them, but I think they are greatly supported by physiological truths, unless the operation of foreign causes alter the laws of action in muscular parts. It is a subject well worth ample investigation, but as it is rather irrelevant to my subject, and would require more room than the limits of this work would allow of, I shall not discuss it here.

The propriety of denominating and distinguishing the cause from the disease, I presume will be admitted. In the disease under consideration, the cause is the poison, and the disease is that action produced in the irritable fibres of the small arteries which may, in the first instance, be clearly discovered; and I think, during the disease, ought to be kept in mind; the one producing, and the other being produced.

If these remarks are well founded, we shall have much reason to doubt the opinion of diseases continuing themselves from their "specific quality."

CHAPTER III.

PHYSIOLOGY APPLIED TO EXEMPLIFY THE CONTINUATION AND EXTENSION OF THE VENEREAL DISEASE.

IT seems to be a law in the action of irritable moving fibres, only to move when stimulated. When that cause is taken away, they remain in a quiescent state, and only again resume the active state when acted upon by cause. The muscular tubes called arteries, continue to act while the blood flows into them, and they retain their power; but when the course of the blood into an artery is checked, the artery remains at rest.*

The venereal poison acts much upon the arterial part of our system. Does that law which governs the healthy state, influence the diseased one? Or does an effect once produced by the venereal poison, continue (without further influence from its cause) until the individual be destroyed, or it be counteracted by the use of medicine?

I have endeavoured to prove that the same poison causes gonorrhœa and chancre, and that it is an accidental circumstance which I shall hereafter

* See note, p. 58.

endeavour to explain, when the effect is gonorrhœa, that it often terminates without the use of medicine. What inferences do these circumstances justify?

If the poison, when brought in contact with the vessels of the urethra, exert its appropriate influence, and these vessels act according to that cause; or if the poison produces in them the venereal inflammation, and that venereal inflammation itself, be able "to keep up itself," *why* does the disease now vanish? I confess I can conceive *no* just or plausible reason: on the contrary, I think this circumstance *alone* is sufficient to prove the invalidity of Mr. Hunter's opinion, because we have the venereal inflammation, but the disease disappears; therefore it cannot be the venereal inflammation, which continues itself.—The fact is, there was a cause for its continuation, which cause being removed, the disease terminates.

If the hard substance, which forms the basis of a recent chancre, be destroyed with caustic, or accurately dissected away, the sore commonly heals without the use of mercury, though a great portion of the part, which is affected by the venereal inflammation, still remains, and matter continues to be formed for several days.—How
does

does this incident happen? Why does not the venereal inflammation here continue? The reason is evident—the poison is taken away—the venereal inflammation is not able to continue itself without its medium, and the disease vanishes.

Mr. Hunter, perhaps, saw such objections as have been pointed out, yet, in order to support his opinion, attempts to elude one of these facts, by a mere ungrounded supposition. In page 35, we find the following opinion:—
 “ But this difference between spontaneous and
 “ non-spontaneous cure, seems to depend more
 “ on the difference in the two modes of action,
 “ than on the difference in the two surfaces; for
 “ when the disease produces an ulcer on a secret-
 “ ing surface, which it often does from the con-
 “ stitution, as on the tonsils, it has no disposition
 “ to cure of itself; nor in the urethra, in a recent
 “ case, if ulcers are formed there, would they
 “ heal more readily than when formed in any
 “ other part.”

This is nothing more than *hypothesis*: it has no other evidence to support it, than a presumption of the validity of his former opinion, which appears to be little more than an unsupported assertion. I shall endeavour hereafter to prove, that

this supposition of Mr. Hunter's, is contrary to, is inconsistent with what he has previously supported.

If we admit of this kind of explanation, we must imagine that a cause is capable of acting, of producing effects in a *variety* of ways. For instance, the venereal poison at *some* times, and upon *some* parts acts like other substances. This we have no foundation for. I have no conception of its acting like any thing but venereal poison, and what variations are observable in the effects excited in the irritable parts, depend upon the *uncertain* and *changeable* state of the living fibre. It is true, that the venereal poison excites different degrees of inflammation in different persons and parts; but that one of these should be venereal, and another an effect of a different kind, I have no idea of—they are *all* venereal—the same cause always *tends* to operate in the precise same way, though the effect may be of different degrees.

Thus the same degree of heat will produce different degrees of effect, according to the then existing state of the irritable parts to which it is applied, yet it always tends to act as heat, never as any other thing.

At one time, inflammation may be the consequence; at another, the same degree may produce mortification: but this is not owing to *different modes* of action in the operating cause, but to the *different states* of the *thing acted upon*. The degree of effect is accounted for, and produced by the variation in the susceptibility of the part.—The heat does not act at one time as heat, and at another time as cold—it always operates in its own *peculiar* manner.

If, when the venereal poison is brought in contact with the urethra, a discharge of poisonous matter is the produce, what reason have we to suppose, that the effect, that inflammatory action which produced it, was not venereal?

If, then, the venereal disease cannot be kept up, and extend from any quality existing in itself, to what cause, or causes, are we to attribute these circumstances?

I am clearly of opinion, we must ascribe them to the influence of the venereal poison.

The poison produces that venereal inflammation in the part, which *constitutes* the disease. The operation of no other cause, which we have knowledge of, will produce the *same* effect. It

is therefore a *peculiar* effect, produced in the small arteries of the part by the immediate concurrent operation of the venereal poison.

If, then, this effect which is alone produced by the venereal poison, cannot support or continue itself agreeably to the principles of action of irritable parts; but requires the immediate or actual presence of another *medium*, in order that the disease may be prolonged; and if the effect be *inimitable*, then no other cause can accomplish this end, than the actual presence of the venereal poison. The original poison then produces the disease, and the new formed *continues* that effect.

The venereal disease cannot be communicated without the intervention of venereal poison. The non-affected part cannot become diseased without the *absolute contact* of that poison.—The adjoining parts *can no more become* diseased without the occurrence of this cause, than an untainted person *can, without* exposure. Therefore, the disease must be *extended* by that power.

In this respect it may be observed, that I coincide, in some measure, with Mr. Hunter, that without the *presence* of the venereal poison, no venereal disease can be communicated.

In page 11, Mr. Hunter says, “ The formation of matter also, though a very general, is not a constant attendant on this disease ; for we sometimes find inflammation produced by the venereal poison, which does not terminate in suppuration ; such inflammation I suspect to be of the erysipelatous kind. It is the matter produced, whether with or without inflammation, which alone contains the poison ; for without the formation of matter, no venereal poison can exist. Therefore a person, having the venereal irritation in any form not attended with a discharge, cannot communicate the disease to another. To communicate the disease therefore it is necessary that the venereal action should first take place ; that matter should be formed in consequence of that action ; and that the matter should be applied to a sound person or part.”

If a surface partaking of the venereal inflammation, without the smallest particle of poison upon it, were to be brought in contact with a sound healthy part, the disease could not be communicated ; that *contact*, without the poison, could not contaminate a sound part. If it were possible to be certain of getting the urethra, and other parts, clear of the smallest particle of the poison of clap, I should have no hesitation of al-

lowing my patients the same indulgence Mr. Hunter di dhis.

The conclusions I have adduced, are from physiological reasoning, and from a few facts, which, I trust, if faithfully considered in an unprejudiced manner, will be found to justify, nay, inform us, of their truth. I have concluded, that the venereal inflammation is not able to continue itself from its specific, or peculiar manner of acting: because it is contrary to the existence of disease in general: and, also, because when this effect is produced in the vessels of the urethra, it vanishes not without a cause, but without the operation of medicine. And am I not authorised to deduce these conclusions? If we have that specific effect (if the expression be allowed), I mean that effect produced by the poison, which nothing else can imitate, and that effect vanishes, does it not evince to us the inefficacy, or the want of power of that effect, to continue itself? Does it not amount to a full elucidation of the fact?

Supposing a venereal chancre was to be produced by its cause, all the poison immediately taken away, and no new poison formed, what must be the consequence? The venereal inflammation could not extend itself, and contaminate
sound

sound parts ; its progress would be immediately stopped ; the part so diseased, that it could not maintain its integrity, would be decomposed ; that which was less affected would recover ; and the breach would be partly filled up, and cicatrized by the vessels around.

The venereal poison, when brought in contact with the prepuce, or glans penis, produces a kind of inflammation, ulceration, and a formation of new poison ; the effect is afterwards continued by the operation of that new formed poison. Dissolution and decomposition* take place in the much inflamed solid particles, by which the poison is brought in contact with fresh parts, which by its influence become diseased. These parts, in their turn, from the violence of the inflammatory action, are deadened, and the same retrograde process commences.

The quantity of poison first applied, would appear, in most instances, to be comparatively small ; the effect and produce, great. In this complaint, a new phenomenon is presented—a product of an effect bearing an exact resemblance to the cause which excited it. I call it a new *phenomenon*, because this is only the case in a few complaints, to which the human body is incident.

* Dr. Marshal's Lectt.

* Mr. Pott.

We find, in general, the cause, effect, and product, which operate on, and in the human body, not to bear the slightest analogy to each other. The effect produced upon irritable parts, is *disproportionate* to the momentum of its cause. —It is not only disproportioned, but it is *various*; at one time great, at another, comparatively small.

The product of an effect is also *disproportioned*, and *unlike* the cause which occasioned the effect. —The prick of a fine needle may occasion a great inflammation and suppuration. The suppuration has not the *smallest* similarity to the cause which produced the disease. If heat, cold, sulphuric acid, &c. produce a disease, the same may be said; the cause and product of the effect has not the smallest resemblance to each other. Thus, then, the animal body, when acted upon, acts in a manner peculiar to itself, and the product of that action has no necessary analogy to the cause.

Admitting then what has been said, we must allow one or other of the two following conditions to take place.—That the poison is *increased*, *multiplied*, or *assimilated*; or that the poison is *incapable* of dilution; that the smallest imaginary particle, when mixed with a great quantity of pus,
or

or other fluids, will have the *same ratio of effect* as a large quantity. This, I think, is so extravagant, so contrary to observation and experience, that no scientific mind can allow it.

Before I proceed further, I would beg leave to assert, that the *poison* or *morbific* matter, when applied to the healthy human body, has the power of producing inflammation in the arteries of the part; that the product of the inflammation is great beyond all calculable proportion to that originally applied; and that it partakes of the nature and qualities of the cause which excited the disease; so that by the application of the poison to the human body, there is an increase, a multiplication, or an assimilation, of new venereal poison.

How this great change, call it multiplication, assimilation, or what you like, is accomplished merits attention.

CHAPTER IV.

A REVIEW OF MR. HUNTER'S THEORY OF THE PRODUCTION OF VENEREAL POISON, &c.

THE opinion, that fermentation caused the matter of venereal sores to be poisonous, I believe is generally abandoned. Though it may be difficult to explain the fact with any degree of consistency, upon the principles of fermentation, yet I can conceive that a number of changes may happen in our bodies, principally produced by new arrangements, and chemical attraction, of the constituent parts of our fluids. Nor does it appear to me absurd to suppose that chemical changes are effected on the surfaces of sores or the sound body.

Mr. Hunter rejects this idea, and supposes that the matter becomes poisonous from a specific mode of action in the part contaminated. But before we take this *theory* into consideration, it is necessary to prepare ourselves by a few preliminary remarks.

For a long period, a vulgar error seems to have pervaded the medical world, respecting the combination of different diseases. Even at the present day, we frequently hear of pocky itches, rheumatic gouts, &c. To Mr. Hunter I feel myself indebted,

indebted, and with pleasure I express the obligation, for the useful and important hints he has given upon this subject. According to him, when the same body or part is acted upon by two different causes at the same instant of time, that body or part cannot be influenced and become diseased, according to each respective cause, at the precise same period. But, as I fear he has not been well understood, and as I know he has been opposed by able men, perhaps, for no other reason, I shall insert some of his own words, and make a few observations thereupon.

Page 2, Introduction, “ The venereal disease
“ is not only suspected to be present in many cases
“ where the nature of the disorder is not well
“ marked, but it is supposed that it can be com-
“ bined with other diseases, such as the itch and
“ the scurvy. Thus we hear of pocky itch, and
“ of scurvy and the venereal disease combined;
“ but this supposition appears to me to be found-
“ ed in error. I have never seen any such cases,
“ nor do they seem to be consistent with the prin-
“ ciples of morbid actions in the animal œconomy.
“ It appears to me, beyond a doubt, that no two
“ actions can take place in the same constitution,
“ or in the same part, at one and the same time.
“ No two different fevers can exist in the same
“ constitution; nor two local diseases in the same
“ part

“ part at the same time ; yet as the venereal
“ disease, when it attacks the skin, bears a re-
“ semblance to those symptoms which are vul-
“ garly called scorbutic, they are often suppo-
“ sed to be mixed and to exist in the same
“ part.”

Mr. Hunter here clearly expresses that, “ no
“ two actions can take place in the same constitu-
“ tion ; no two different fevers can exist in the
“ same constitution at one and the same time,” by
which I understand him, two distinct appropriate
modes of action cannot take place universally
in the moving fibres of the heart and arteries
at the same time.

It is a direct contradiction, and, therefore, *cannot*
be true, to suppose that one vessel, or any
set of vessels can move at the same time, in two
different ways, or act conformably to two differ-
ent causes at one time. It can no more happen,
than a man can travel to two *opposite parts* of the
globe at once. Place a man upon the equator,
with motives to go towards the *two poles* ; can he
accomplish those two desired ends by moving
in the same tract ? No, if his motives for going
into the northern hemisphere be the strongest, he
directs his course towards that place, and reaches
that

that point; after which he may go into the southern hemisphere, if he pleases.

Mr. Hunter also says, "that no two actions
 " can take place, in the same part, at one and the
 " same time."—If *two* causes be applied to a
 living artery, the properties of each being to
 produce *different* degrees of muscular action, that
 artery *cannot obey both*.—If you apply opium, the
 operation of which is to *still or quiet* the vessel,
 and, at the same time spirit, to *increase its action*,
 the artery cannot be influenced according to both;
 it cannot be *still*, and *move* with great force at the
same instant; but the two may, in some measure,
 counteract each other, and an effect may be pro-
 duced, *balanced* between them, and *different* to
 that which either must have produced, had it been
singly applied.

In the next paragraph, he says, "A man may
 " have the pox and small-pox at the same time;
 " that is, part of his body may have been conta-
 " minated by the venereal poison, and the small-
 " pox may take place, and both diseases may ap-
 " pear together, but not in the same parts. If both
 " were consequences of fever, and each followed
 " the fever nearly about the same time, it would be
 " impossible for each to have its respective erup-

“tion, even in different parts, at the same time ;
“two fevers, antecedent to these different dis-
“eases, cannot be co-existent.”

This circumstance he illustrates by a very pointed case, where the small-pox fever, and eruption, were suspended by the measles.

A child cannot have the fever of small-pox and measles, with their respective eruptions, at the same time, that which first took effect must decline before the other can appear. If a child should be exposed to the *contagion* of these two diseases, so that the fever, according to the *operation* of their causes, should each appear on the same day, they could not *both* operate ; the heart and arteries could not be acting, according to each respective cause, at one and the same time. The cause to which there was the *greatest susceptibility* in the child's constitution, would act first, and the eruption *peculiar* to it would first appear ; and after this fever had *wholly* or *partly* subsided, the other might operate. It is totally impossible, that the heart and arteries could have two distinct modes of action at once : nor can one and the same fever produce two *different* eruptions, yet the eruption of each may be present upon the body at the same time.

It appears also equally true, that one disease may *disappear*, by the attack of another; or that one cause may cease to operate upon the application of another to which the body or part has a *greater susceptibility*, and the previous disease *may or may not* be cured.

For instance, a scrofulous sore may, by the application of venereal poison, become venereal—the *cause of scrofula* ceasing to produce its effects from the greater force in the *venereal cause*; or a *greater susceptibility* to it in the living irritable parts. By the operation of mercury, the venereal effects may be counteracted, and the ulcer heal as a common venereal sore, or may again become scrofulous.

Mr. Hunter allows, that the human body may have two different diseases upon it at the same time, but upon different parts. In short, his observations appear to me so just upon this subject, and his arguments so clear, that I am surprized that the former should be doubted, or the latter misunderstood.

After reading attentively this section of his introduction, we cannot be a little surprized to observe his definition of inflammation, which is, at least, incompatible with his observations upon

morbid actions. “ I consider common inflammation to be an increased action of the smaller vessels of a part, joined with a peculiar mode of action, by which they are enabled to produce the following effects; to unite parts of the body to each other; to form pus; and to remove parts of the solids. These effects are not produced by a simple increase of action or enlargement of the vessels, but by a peculiar action, which is at present perhaps not understood.”* Does not this definition combine two modes of action at the same time, in the precise same part? The *common action of the artery* increased, and Mr. Hunter’s “ *peculiar action.*”

Mr. Hunter then accounts for that particular action in the small arteries of a part which constitutes, what social convention terms inflammation in an inconsistent manner—inconsistent with what he already has said, in his section upon morbid actions, and, I humbly think, inconsistent with those principles of action which govern irritable parts.

Let us now return to our former subject, and carefully attend to the following quotation, and, I am persuaded, we shall find still greater inconsistencies.

* Hunter’s Treatise, Page 6.

“ We know nothing of the poison itself, but
“ only its effects on the human body. It is com-
“ monly in the form of pus, or united with pus,
“ or some such secretion, and produces as similar
“ matter in others, which shows that it is most
“ generally, although not necessarily, a conse-
“ quence of inflammation. It produces, or ex-
“ cites therefore, in most cases, an inflammation
“ in the parts contaminated; besides which in-
“ flammation, the parts so contaminated have a
“ peculiar mode of action superadded, different
“ from all other actions attending inflammation;
“ and it is this specific mode of action which
“ produces the specific quality in the matter. It
“ is not necessary, that inflammation should be
“ present to keep up this peculiar mode of action,
“ because the poison continues to be formed long
“ after all signs of inflammation have ceased.
“ This appears from the following facts: men
“ have only what is called a gleet or healing chan-
“ cre, give the disease to sound women: and
“ many venereal gonorrhœas happen without
“ any visible sign of inflammation.” Hunter’s
Treatise, p. 11

It is to be lamented, that Mr. Hunter has not been more precise in the explanations of his respective theories. This defect exposes them to be misunderstood, and, perhaps, misrepresented, each

person giving an explanation according to his own way of seeing them;—we are certainly left much in the dark, and can only come at a probable conjecture by careful inference: therefore, I hope for pardon, if my inferences should not be such as the world approve of.

In what part, or parts, Mr. Hunter imagined the “specific mode of action” to take place, ought not to be hastily presumed: but, by his previous definition of inflammation, we may induce it to be in the small vessels of the part. The next deficiency to be supplied by inference, is in what consists this specific action? In the particular mode of muscular action in those vessels? Or in an action of a totally different kind of which the vessels are capable? Though the word specific itself expresses nothing, and every cause and effect may be called by that term, yet I think, his explanation implies an action not muscular; for he says, “besides which inflammation, the parts so contaminated have a peculiar mode of action super-added, different from all other actions attending inflammation,” and inflammation he has previously described to consist in an “increased action of the smaller vessels of a part, joined with a peculiar mode of action;” therefore, I think, we may fairly conclude the “specific mode of action,” to be an action in the small vessels
of

of a different nature to their common and well known action of which they are capable from their muscularity.

If this were the opinion of Mr. Hunter, and which I know to be the opinion of some of his adherents and cordial supporters, I would beg leave to observe, that it appears altogether *hypothetical*. I know not, nor can I conceive of any action in those muscular tubes, but what they are capable of, from their particular arrangement of muscular fibres, neither can I conceive we are authorised to admit of it, until demonstrated.

In most cases he allows that the venereal poison excites inflammation, “ besides which inflammation, the parts so contaminated have a peculiar mode of action superadded, different from other actions attending inflammation.” In this instance, does it not clearly appear, that Mr. Hunter combines in the *same part*, and at the *precise same time*, three distinct modes of action? “ The increased action of the smaller vessels,” the “ peculiar action of the inflammation :” these two together combined, constitute (agreeable to his words) inflammation. To these he adds a third, “ the peculiar action” attending venereal inflammation. These three different actions in the small vessels of the part, according to his explanation,

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planation, comprise the disease. And all produced from the same cause.

He tells us, that "the poison is in the form of "pus, or united with pus, or some such secretion;" this is vague information, and implies the same inconsistencies and contradictions as his other assertions. For if it be "mixed with pus" (which is a product of inflamed vessels), it therefore, according to him, is formed by the "increased and peculiar modes of action" in the vessels, and which constitute what is denominated inflammation; to which is added, the "specific mode of action which produces the specific quality of the matter." Here then we have the same three actions combined.

But if it be united with pus, or any other secretion, then at least he implies two distinct effects from the same cause, existing in the same part at the same time. The secretion, he would say, "is produced by a peculiar action in the arteries secreting;" to which is added, the peculiar, or specific venereal effect.

He tells us, that "the poison is a general, "though not a necessary consequence of inflammation." If, then, it be a product of inflammation, we want no farther explanation; but he
afterwards

afterwards contradicts this information, and says, “ that it is produced by the specific mode of action.”

In a number of particulars in this section, Mr. Hunter not only disagrees with himself, but, I think, with the principles of disease, in moving irritable parts: for, if two causes were to be applied to a set of living muscular vessels, these vessels could not act in conformity to both, and at the same time; much less is it possible that they could have three modes of action, at one and the same time, from the occurrence of one cause.

We may collect, from what Mr. Hunter has said in this section, the following extraordinary assertions. That the poison is “ most generally a consequence of inflammation.” That “ it produces or excites in most cases, an inflammation in the part contaminated;” that it also produces, in the same part, and same time, a “ specific mode of action which produces the specific quality in the matter;” that the “ inflammation and suppuration, when present, are only attendants on the peculiar mode of action;” and that the poison sometimes produces erysipelatous inflammation.

To reconcile these different opinions to the laws of causes and effects, will, I apprehend, be a very tedious and difficult task.

Mr. Hunter, then, to give sanction to his own opinion, controverts and rejects that of fermentation, and, it appears, establishes his own upon its ruins. We certainly are obliged to any man who will take the trouble of shewing us the inefficacy of any supposed *theory*, to explain the known effects of any disease. But though this should be completely attained, a different one is not thereby established. The new one ought, nevertheless, to be such as shall be justifiable by facts and observations.

Mr. Hunter, in his section "Of the Causes of Poisonous Quality," p. 17, takes up this question thus: "As the consideration and explanation of
 " this point will throw some light upon the dis-
 " ease, and cure, I may be allowed to dwell a little
 " upon it. It has been supposed by some, that
 " the poisonous quality of the matter arises from
 " a fermentation taking place in it as soon as it is
 " formed. But whether this poisonous quality
 " arises from that cause, or whether the animal
 " body has a power of producing matter accord-
 " ing to the irritation given, whereby the living
 " powers, whenever irritated in a particular man-
 " ner,

“ner, produces such an action in the parts as to
“generate a matter similar in quality to that
“which excited the action, is what I am about
“to consider.”

This view of the question cannot but excite fresh surprise. Mr. Hunter has now taken entire fresh ground. He deserts, or disregards his previous assertions made in the section upon the nature of the venereal poison, and now considers it as a law in the action of irritable parts (when matter is the consequence), to be such as will produce matter of a similar quality to the cause. How far this is consonant with observation, I will make some inquiry.

In the first place I would observe, the question as stated by Mr. Hunter, would be just, provided there be no other possible way of accounting for the effect, than the two means here pointed out. Then, if Mr. Hunter shewed the fallacy of fermentation, the other must be admitted. But this I do not conceive to be the case.

Mr. Hunter professedly confines himself to gonorrhœa, in the examination of this subject; this, it would appear, is not perfectly equitable, because, some of the circumstances which attend this effect of the poison, are accidental; not
owing,

owing, as I before have said, to the qualities of the poison, but to the nature of the part affected.

Without entering, at present, into the examination, whether the opinion of Mr. Hunter is consistent, it may first be proper to take a view of its efficacy, in explaining some of the well known circumstances and incidents of the venereal disease. If in this respect it should be found deficient, we shall have reason for doubting it.

In this examination, it cannot be thought unfair to take some of those occurrences, offered by the founder of the doctrine in its favour ; these, no doubt, being in his mind the most likely to illustrate and establish his opinion.

“ First, the venereal matter having a greater
“ power of irritating than common matter, con-
“ veys more the idea of irritation than of fermen-
“ tation.” Page 19.

The venereal poison having the power of producing a disease (call it irritation, if you please), is not doubted ; but this conveys no idea from which we could expect that the product of this irritation would be poisonous : a variety of substances will irritate, but the produce of these effects

effects is not thereby necessarily, like the cause which produced it.

“ Secondly, its producing a specific disease
“ with specific symptoms and appearances, shows
“ that it has a specific power of irritation, the
“ living powers necessarily acting according to
“ that irritation.”

Every substance we are acquainted with, is *specific*, and no doubt produces its effects in a manner *specific*, or *peculiar* to itself. But this shews no argument in favour of Mr. Hunter's opinion. The effects caused by the *specific qualities* of the respective kinds of matter, *does not* generate such respective substances. This will be more fully discussed elsewhere.

“ Thirdly, the circumstance of the inflammation having its stated time of appearance and
“ a termination, is agreeable to the laws of the
“ animal œconomy in most cases, as it is a circumstance that takes place in other diseases that
“ have crisis; and when the disease is longer of
“ duration in some than in others, it is because
“ they are much more susceptible of this kind of
“ irritation, and there may be perhaps other
“ current circumstances.

The time of appearance and termination of gonorrhœa is extremely various, neither of which can be stated. Chancre in its appearance is extremely different; and often only terminates with the life of the individual. But supposing these incidents could be exactly pointed out, they would afford no proof or support to Mr. Hunter's opinion.

“ Fourthly, the venereal inflammation being
 “ confined to a specific distance, is more agree-
 “ able to the idea of specific irritation, than that
 “ of fermentation.”

The venereal disease, if not counteracted, will seldom confine itself to the organ it first affects, but will spread and communicate to almost every part of the body. The bones, skin, throat, nose, &c. are subject to its ravages; the specific distance is only the *utmost* extent of the body. When the disease is gonorrhœa, the whole length of the urethra is often affected.

“ Fifthly, we have a farther proof of this opi-
 “ nion, from the appearance of the disease being
 “ translated from one part of the body to another,
 “ as in the case of swelled testicle, in which the
 “ discharge is often stopped or otherwise af-
 “ fected.”

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How diseases are *translated* from one part of the body to another, I have no idea. I have no conception of such wonderful, such monstrous effects. I know of no *translating agent* in the human body. I have frequently seen a swelling take place in one or both testicles of a person affected with a clap, which clap has ceased, for a time, or altogether. But this effect is falsely accounted for by the term translation. It conveys false ideas upon the fact, therefore ought not to be used. It implies an agent employed to accomplish that end. Though the seeds of disease may be conveyed from one part of the body to another, a disease, as it stands, cannot be translated.

In this occurrence, the disease in the testicle, from what Mr. Hunter styles translation, is generally, if not universally, allowed not to be venereal. Nay, Mr. Hunter himself asserts it is not. If this disease in the testicle should terminate in suppuration, still the disease is not venereal—the product not poisonous. How could this happen if the disease were translated? If the complaint were removed from its seat in the urethra to the testicle, the product of that effect must be as poisonous as when in the first seat.

“ Sixthly, the discharge often stops from the
“ constitution being attacked by fever, and re-
“ turns

“ turns after some days or weeks, or not at all,
“ according to the continuance of the fever.
“ Now we can plainly see, why the fever should
“ put a stop to the discharge, as the disposition,
“ produced by it in a part, is very different from
“ that disposition which formed the matter; and
“ we can plainly see, why the same disposition
“ to form matter should often return; but how
“ the return should be venereal, upon the principles of fermentation, we do not see.”

I would hope, for science's sake, that this *shaded* manner of accounting for disease is at present disregarded. Is it possible to see plainly, how a part diseased can become well, and again diseased from *disposition*? Are such explanations admissible without the smallest proof of the existence of mind in that part? No, we must look for other causes, and, in this particular disease, and incident, we, I think, must admit the presence of the poison.

If the venereal poison produce the peculiar action in the vessels of the part, which action constitutes the true disease, and is preparatory to the formation of poison, and if there is no other cause which will produce the same effect, it is impossible that the same disease could again recur, or the same vessels again become similarly diseased

eased without the same cause. Suppose the disease to be extinct but for one hour; that the action which was excited by the actual contact of the poison had ceased; and that no poison was present, could the part, without any cause, fall into the venereal disease? It is *impossible*; for a disease, in no instance, takes place, without the occurrence of its cause: If I were to ask, is it possible that a person not exposed to the venereal contagion, could have that disease? The answer would immediately be, no, impossible! Then I would say, it is equally impossible, if the disease has ceased for one hour, that it can again return without the immediate operation of its cause. It does not make the smallest difference respecting the question, whether the cause has been removed, and the disease terminated one hour or one year; it cannot again commence without its cause.

Gonorrhœa ceasing without the aid of medicine, is an accidental circumstance; but, if the poisonous quality of the matter arose from the peculiar action in the vessels which form it, how could this casualty happen? Why not continue as in chancre, or any other effect of the poison? For we know that the vessels of the urethra are *susceptible* to the influence of the poison, and capable of *continuing* the disease, which is evinced when chancre forms on that surface.

I will mention one more circumstance still further to prove the inefficacy of Mr. Hunter's opinion. If, at an early period, the matter of chancre be washed away, or otherwise carefully removed, and the hard substance which forms its basis, be destroyed by caustic, or accurately dissected away, it is reduced to a common sore, and heals by common remedies, notwithstanding a large portion of the inflamed part remains, and matter continues to be formed for some days. Here then you have Hunter's "specific action" brought on by the "specific quality of the poison;" but not the poison for a produce, which certainly would be the case, if Mr. Hunter's opinion were true.

We are, therefore, obliged to suspect Mr. Hunter's theory, because we cannot, by its aid, explain satisfactorily many of the phenomena of this disease.

Let us now take up the question in another manner—Let us inquire how far Mr. Hunter's opinion is consonant with our knowledge of cause and effect upon the human body—Let us inquire, whether a cause acting upon the human body, has the power of producing an effect, which shall cause a product necessarily possessing qualities

ties exactly resembling the cause which excited that effect; or, in other words, agreeable to Mr. Hunter, “whether the animal body has a power
“of producing matter according to the irritation
“given, whereby the living powers, whenever
“irritated in a particular manner, produce such
“an action in the parts as to generate a matter
“similar in quality to that which excited the ac-
“tion.”

In the human body a number of organs is placed, the functions of which are to elaborate peculiar kinds of fluids for particular purposes to the animal œconomy. These organs, which are called secreting viscera, are principally composed of a congeries of blood vessels. Some of the arteries which go to these organs end in veins by continuity of canal, and propel their contents by their muscular action into these veins. Others, it would appear, have open extremities, and, by their action upon their respective fluids which they circulate, *elaborate* peculiar products, according to the nature of that part. *

If the product of any of these secreting viscera be inspected as a homogeneous mass, it appears to be totally unlike the blood from which it was elaborated, though, I believe, according to the most ac-

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* Dr. Moore's *Lectures*

curate analysis, the *ingredients*, the *constituent principles* existed previously, though in a *different* state of combination, in the blood; so that it is *only* a new compound formed out of the *constituent parts* of that fluid, probably by *new* arrangements, chemical attractions, and combinations: *no new constituent part is thereby formed.*

Nearly the same, I believe, will be admitted, provided any part of the arterial system be acted upon by a cause which produces inflammation and suppuration. This product will be *elaborated* from the blood, or constituent parts thereof, which the arteries circulate, and a *new* compound is formed out of those parts. The product is elaborated *by the disease* in the small arteries, and *from the fluid* which these arteries circulate: *no new constituent principle is here generated*.*

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* Mr. Hunter, in his valuable Treatise on the Blood Inflammation, &c. ch. 5, Of Pus, seems to think, that a new arrangement of vessels takes place in the part inflamed, and that it becomes glandular; and the product he calls a secretion. His own words are, "Pus is not to be found in the blood, similar to that which was produced in the first stage; but is formed from some change, decomposition, or separation of the blood, which it undergoes in its passage out of the vessels, and for affecting
" which,

In this instance, then, the cause, be it what it may, alters the state of parts, and that degree of muscular action in the small arteries which constituted health. This action in the arteries being upon their contents which they circulate, a product

“ which, the vessels of the parts have been formed, which
“ produces a subsiding of the inflammation from which it
“ took its disposition; hence it must appear, that the form-
“ ation of pus consists of something more than a straining
“ of juices from the blood. Many substances indeed
“ which are to be considered as extraneous bodies in
“ the blood, being only mixed with, and not making an
“ essential part of that fluid, and perhaps even necessary
“ to it, may pass off with the pus, as with every other
“ secretion, yet the pus is not to be considered on that
“ account as simply parts of the blood unchanged; but
“ we must look upon it as a new combination of the blood
“ itself, and must be convinced that in order to carry on
“ the decompositions and combinations necessary for pro-
“ ducing this effect, either a new or peculiar structure of
“ vessels must be formed, or a new disposition, and of
“ course a new mode of action of the old must take place.
“ This new structure, or disposition of vessels, I shall call
“ glandular, and the effect or pus, a secretion.” p. 416.

Without making any observations upon Mr. Hunter's mode of reasoning, his conclusions, perhaps, come as near the truth, as our present state of knowledge will lead us to; and, I think, bring us to that important point from which we must set out, if we attempt to expound those great and important secrets in the animal œconomy, which have not only eluded, but astonished our ancestors.

is elaborated, and from the constituent principles of the circulating fluid, a new compound is formed. Nor does this new compound, as I before observed, necessarily bear the smallest resemblance to the cause, either in appearance or properties.

If, for instance, sulphuric acid be applied to the skin, a formation of matter may be the consequence. But this matter *is not sulphuric acid*. The same may be said upon every other substance; caustic, alkali, arsenick, &c. the effect excited by these applications, *does not* generate a matter similar in quality to that which caused the effect: *not arsenick, sulphuric acid, &c.* Though each of them irritate in a manner *peculiar* to themselves, the product elaborated by the vessels thus caused to act upon the fluids which they circulate, possesses no similar quality to the cause, except it be mixed with those substances before or after leaving the vessels.

The human body is not a soil, to receive, germinate, and propagate the seeds of disease; without this multiplication of causes the human body is beset with an indefinite number, by a repetition of the effects of which its existence is soon terminated. But had it the power of propagating these destructive evils (for such they are, in fact, to the animal body), its existence would be infinitely more short and miserable.

It perhaps may be said, that this view of the subject is too comprehensive, and that Mr. Hunter did not extend the explanation so far, but confined it to a few morbid complaints brought on by poisonous causes, which act, as he would stile it, in a “ specific manner.”

To this I would answer, I know of no *cause or thing more specific* than another ; the operation of every one is *peculiar*. The term specific will equally apply to every substance in nature. Every cause, it would appear, produces its effects in a manner peculiar to itself, and unlike that of any other. And although a necessary variation is effected, by the alteration produced in the structure of the part, and the degree of effect on the irritable fibre, and hence the product will be somewhat various, yet it *always* arises from the constituent principles which are in the blood, and nothing new is thereby inserted. The general laws which govern the action of those irritable parts are not confuted.

Seeing then that every cause operates in a particular manner, and, of course, that the effect and product are in some measure diversified agreeably to that peculiarity, variations will occur without end: yet thus much may be said generally, that *whatever* variation can take place, *whatever* different product can be caused by this peculiarity,

is the *result* of combinations from the ingredients which compose the blood ; or with which it may be impregnated, or in the solid parts which may have suffered dissolution or decomposition. The compound is only new, not the parts thereof.

Thus I submit, that Mr. Hunter's opinion is inconsistent with the general effect upon irritable parts. That the product of inflammations is not necessarily and generally of the same nature and qualities as the cause which produced that inflammation ; on the contrary, almost always dissimilar.

CHAPTER V.

PRODUCTION OF VENEREAL POISON.

THIS is a subject equally interesting and curious; it throws much light upon disease, and the œconomy of our bodies; it may not, therefore, be thought impertinent to dwell a little upon it.

The first opinion of fermentation is objectionable, because it does not take into account sufficiently the effect produced upon the solid irritable fibre, and which, we may conceive to take place in dead animal matter.

Mr. Hunter's (the second) opinion, I think, is objectionable, because by it I am unable to explain many incidents which commonly occur in this disease, and also because it appears to be in defiance of laws observable and unalterable in the animal œconomy.

To conduct us a little nearer to our object, it may be observed, that the symptoms of this disease appear in a longer or shorter period after the exposure according to circumstances, and that the effects

effects of this disease, are varied according to the nature of the part diseased.

In gonorrhœa, a discharge of matter flows from the urethra of different consistence and colour, attended with particular symptoms, owing to the nature and function of the part.

The discharge, as previously observed, is produced by the disease in the capillary arteries of the urethra, and, I am of opinion, flows from these vessels a harmless fluid. It undergoes a visible alteration after leaving these vessels; for if a patient, labouring under this complaint, be directed to make water, or, by other means, clear the urethra of the collected matter, and the part be gently pressed, and examined a few minutes afterwards, a thin watery, transparent fluid appears, which, if allowed to remain an hour or two, becomes thicker and opaque.

When the poison produces its effects in the urethra, there is, in general, no dissolution of the solid particles of the part. When this complaint is only local, and the blood not at all contaminated, the product is elaborated by the disease in the small arteries, and out of the constituent parts of the untainted fluid which they circulate; therefore, as concerned with the muscular

cular effect in these arteries, one could hardly expect it to be poisonous.

If the poison has been applied to the glans penis, prepuce, or a part having a dense dry skin and cuticle, after an uncertain period, symptoms of inflammation appear, and almost as soon ulceration. The matter here found is poisonous: it is formed out of the decomposed solid particles of the inflamed arteries, veins, nerves, and lymphatics, which enter into the structure of the part with their juices, and the product of the inflamed arteries: these make essential parts of its composition.

These observations hold good whilst the chancre is forming or extending; but when the chancre is stationary (the ulceration not spreading) or healing, then of course those decomposed parts do not make a part of the new compound: it is formed alone out of the fluid which is moving in the inflammatory acting vessels.

It is also worth while here repeating, that chancre is a local complaint, the blood for some time not appearing to be tainted, and that the new compound issuing from the inflamed arteries, is also formed out of the wholesome fluid which they circulate.

The product of the venereal inflammation in this state of the disease, also undergoes an obvious change after leaving the vessels which form it. For if, a short time after dressing a large chancre, it be inspected, the surface will be found covered with a thin, transparent, watery moisture, which, if allowed to remain a few hours, will also become more thick and opaque.

These changes I have repeatedly remarked, and I trust will be confirmed by the observations of others.

To suppose that the poisonous quality of the venereal matter is virtually produced by the particular degree of muscular action in the inflamed arteries, puts us under the necessity of imagining, that the inflammatory mode of acting in the arteries produces, from their untainted contents, a new poisonous compound; and I think, also, *a new constituent, or elementary principle.*

If the other opinion which I espouse, be admitted, that the mixture of the poison with the product of the arteries, makes it poisonous, we go upon the more natural supposition, that the poison possesses some elementary principle not existing in the blood; that it is a foreign substance, foreign with respect to the human body, which has the power of producing inflammation in the vessels of the part to which it is applied;
and

and that this inflammatory action produces a new compound from the fluid which they circulate, which new compound is not poisonous as concerned with the action in the vessels which formed it; but is afterwards assimilated by being mixed with the venereal poison.

These observations, even in their present imperfect state, I venture to throw before the public. —If they should be found to show any appearance of fallacy in Mr. Hunter's theory upon this subject, they cannot fail to produce a good effect. They will, no doubt, be applied in a more able and better manner for the benefit of mankind.

CHAPTER VI.

APPLICATION OF THE PHYSIOLOGY ADDUCED IN THIS
PART.

AIDED by the light of these observations, I shall endeavour to explicate some of the phenomena, and circumstances attending the venereal disease.

First, Gonorrhœa often ceasing without the use of medicine.

Admitting Mr. Hunter's opinions to be true, that the disease is "kept up by the specific quality of the inflammation," and that the poisonous quality of the matter is owing to the "specific mode of action," it would be difficult to conceive how this incident could happen, because the action would continue, and poison would be formed. If it were even to be admitted, that the disease is continued by the poisonous quality in the matter, and Mr. Hunter's opinion respecting the production of the poison adhered to, a great
and

and insurmountable obstacle would still obtrude; because, while the venereal inflammation continued, poison would be formed; cause producing effect, and effect, cause eternally: for we know that the human body never becomes unsusceptible of the influence of the venereal poison. Therefore, without taking into consideration concurring circumstances which are not admitted by Mr. Hunter's theories, we cannot, with any degree of consistency, explain this incident.

From what I before have said, I think we may easily perceive how this casualty takes place. The discharge is produced as previously explained, without a breach in the texture of the part. The inflammation has no power of extension further than what takes place from the injury and influence of the poison—It is copious, owing to the nature of the part—It finds easy egress out of the lacunæ and urethra, and is frequently washed away by the urine. By the concurrence of these circumstances the poison is much diluted, and perfect assimilation, perhaps, is prevented. By a constant and regular succession of these occurrences, the poison is more and more weakened, and ultimately washed away, when the venereal inflammation not being able to support itself, terminates for want of its cause.

Secondly,

Secondly, When a patient, affected with gonorrhœa, is seized with a fever, or other affection of the general system, the gonorrhœa is more or less thereby affected. It often returns after the fever disappears.

The friends of the opinions of Mr. Hunter would explain this phenomenon, by supposing that the fever changed the disposition of the part, and no matter was of course formed, during which time the poison was washed away; but that after the fever had subsided, the same disposition returned, and the extinct disease recurred. This kind of explanation to me, at least, is in a high degree unsatisfactory: it does not throw the smallest light upon the circumstance; on the contrary, much obscures it, neither have we the smallest evidence in favour of such an effort of mind in the part. The disease did not, in the first instance, commence without the incidence of its cause; and if it ever subsides, and the cause be removed, the part cannot again resume the disease, without the concurrence of the same circumstance.

In this instance, the fever alters the susceptibility of the part, and the vessels cease to operate agreeably to its influence. But when the fever disappears, the susceptibility returns—the disease
revives.

revives. If the clap has entirely terminated, and relapses, we must admit some of the poison to have been concealed in the pores of the urethra or lacunæ, or we cannot account for its returning. There is no disposition in the part for the disease, therefore it is absurd, nay, erroneous, to explain it upon that principle. When the fever ends, and the vessels become again susceptible to the influence of the poison, the cause is present, and the effect produced: if this were not so, no disposition could make the part act agreeably to a peculiar and foreign cause, nor can the highest degree of susceptibility produce a disease without a cause.

3dly, Gonorrhœa is often wholly or partly stopped by an inflammation in one or both testicles.

It is no translation, nor is it sympathetic. I know well that the testicles, and parts adjacent, are often rendered tender by the effects of a clap, which make them more susceptible to inflammation. But this complaint, it appears, is nothing different from a common inflammation in that organ. It yields to bleeding, purging, &c. If suppuration is the consequence, the product is not venereal. Indeed, unless the matter found access to the testicle, we could not expect it. This may appear a bare possibility, by its falling down the
I urethra,

urethra, and along the course of the vasa deferentia to the testicles; but, from a number of circumstances, it would appear impossible, and by the disease giving way to common treatment, I think we are warranted to conclude it to be a common inflammation. When this complaint takes place, the vessels in the urethra are rendered less susceptible to the venereal poison, and the clap declines, or ceases, in some cases. The clap may, or may not return, which happens in the precise same manner as explained in fever.

4thly, The venereal effects can not only be arrested when they have appeared, but prevented from making their appearance by the presence of another disease; yet, even in this case, the effects show themselves upon decline of that disease.

In this case, we are bound to admit, that the poison had insinuated itself into the pores of the part; but the part, at the time of the application, not being susceptible to its influence, it remained inactive; but when the previous disease vanished, the part became susceptible, and the disease was produced.

5thly, It would appear, that the product of venereal sores has different degrees of virulence.

This

This fact seems to be incompatible with Mr. Hunter's opinion. For, if the poisonous quality of the matter be caused by the peculiar action in the vessels, one would expect that the poison would have always the same degree of virulence. But, if the matter becomes poisonous after leaving the vessels, we can easily perceive that assimilation may be more or less perfect.

6thly, Mercury has not so powerful an effect in curing gonorrhœa, as chancre; and we can now partly explain the reason.

If we admit with Mr. Hunter, that the poisonous quality of the matter is caused by the action in the vessels, we shall find some difficulty in explaining how this incident happens. Mercury, we should on this theory suppose, would change that mode of action producing poison; poison would no longer be formed; and the disease would disappear by the use of this remedy as easily as chancre.

But, allowing the disease to be kept up by the power of the poison, and that the matter becomes assimilated after leaving the vessels, we can form some rational idea respecting the circumstance.—The poison is deposited in the urethra, its pores

and lacunæ in contact with the external surfaces of the small arteries; hence its effects are kept up, and the waste is supplied by the product being converted or assimilated, as thrown from the arteries and mixed with that poison, notwithstanding the use of mercury be persisted in. The disease is continued by the poisonous quality of the matter, and poison continues to be assimilated.

7thly, It appears, that the matter of gonorrhœa and chancre is infectious after the inflammation has disappeared; after the vessels of the part have ceased to act particularly to the cause.

This I conceive to be a further illustration of the inefficacy of Mr. Hunter's opinion; because, to our senses, the irritation, which the poison excited, has terminated, yet poison continues to be formed. This may happen from the poison having the power of assimilating other compounds besides that which its own qualities had caused to be produced. We can only account for this circumstance, by admitting the assimilating properties of the poison.

9thly, It is found, from experience, that if the medicines used for the cure of a clap, be laid aside

aside as soon as the symptoms disappear, they in general return. How could this happen, if Mr. Hunter's opinion was well founded? We are bound in this case to believe, that some of the poison was left in the pores of the urethra, which again produced its effects.

CHAPTER VII.

PRACTICAL ADMONITIONS:

THE practical admonitions suggested by these observations, are various: great attention to cleanliness, local applications, frequent removal of the dressings, and the venereal matter, &c. &c. These, I venture to pronounce, comprise a part of the curative plan, not the least essential. I am sorry to be under the necessity of adverting to a contrary, and, I think, a most dangerous opinion, asserted by Mr. Hunter, which he seems to make in support of his other opinions, but against experience.

Mr. Hunter observes, " From this circumstance of all gonorrhœa ceasing without medical help, I should doubt very much the possibility of a person getting a fresh gonorrhœa while he has that disease; or of his increasing the same by the application of fresh matter of its own kind. And this observation holds in all the forms of the disease; for it has been proved, that the application of the matter

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“ from a gonorrhœa to a bubo does not in the
“ least retard the cure of that bubo; nor does
“ the matter of a chancre applied to a bubo,
“ nor the matter of a bubo applied to a chan-
“ cre, produce any bad effect; though, if ve-
“ nereal matter is applied to a common sore, it
“ will often produce the venereal irritation. By
“ all which I am lead to believe that the venereal
“ matter formed in a gonorrhœa does not assist
“ in keeping up that gonorrhœa; for it is only
“ an application of matter, the poison and effects
“ of which are exactly similar to the effects
“ upon the solids already produced; and that no-
“ thing could increase or continue the effect but
“ something that is capable of increasing the dis-
“ position of the parts themselves to such inflam-
“ mation, or of making them more susceptible of
“ it. We find besides, that a gonorrhœa may be
“ cured while there is a chancre, and vice versa:
“ now if fresh venereal matter were capable of
“ keeping up the disease, no gonorrhœa could
“ ever get well, while there is this supply of ve-
“ nereal matter*. From all this it is reasonable
to

* “ When treating of pus, in my lectures, I observed that
“ I was inclined to believe that no matter, of whatever
“ kind, can produce any effect upon the part that formed
“ it: nor do I believe that the matter of any sore, let it
“ be what it will, ever does or can do any hurt to that
“ sore:

“ to suppose that such a surface of an animal
 “ body is not capable of being irritated by its own
 “ matter ; nor is it capable of being irritated be-
 “ yond a certain time ; and therefore if fresh ve-
 “ nereal matter were continued to be applied to
 “ the urethra of a man having a gonorrhœa, that
 “ it would just go off as soon as if no such ap-
 “ plication had been made, and get as soon well
 “ as if great pains had been taken to wash its
 “ own matter away. The same reasoning holds
 “ good in chancres*.”

I have not made the experiments of applying
 the matter of gonorrhœa to bubo, &c. but I
 am thoroughly persuaded, that the cure would
 be retarded. It does not appear true, that all

“ sore ; for the parts which formed the matter are of the
 “ same nature, and cannot be irritated by that which they
 “ produced, except extraneous matter is joined with
 “ it. The gland which forms the poison of the viper,
 “ and the duct which conveys it to the tooth, are not
 “ irritated by the poison : and it would appear from Abbé
 “ Fontana’s experiments, that the viper cannot be affect-
 “ ed by its own poison. Vide *Traité sur la Vènin de la*
 “ *Vipere*, par M. F. Fontana, vol. I. page 22. If what
 “ I have now advanced is true, wiping, or washing away
 “ matter under the idea of keeping the parts clean, is in
 “ every case absurd.”

* Hunter, page 35.

gonorrhœas would cure themselves; I have seen some, which have continued long, form chancre. When a gonorrhœa ceases without medical aid, it may with some propriety be termed an accidental circumstance; that is to say, it is not a constant and essential incident attending the effects of the venereal poison; therefore, surely in this instance, it is improper to adduce from it such general conclusions.

I have universally observed a mitigation of all or several of the symptoms of gonorrhœa, by frequently washing the part, and applying clean linen. I have also constantly observed chancres, and other venereal sores, when negligently dressed, (independently of other disagreeable circumstances) cure slower, and require a greater quantity of mercury, than those which were properly attended to. Indeed, Mr. Hunter seems so perfectly convinced of the utility of removing the matter of chancre, that he recommends such sores to be dressed three times a day, and also specifies several local applications*.

We cannot, from fair inference, say, that venereal sores become unsusceptible to the influence of that poison. The accidental circumstance attend-

* Hunter's Treatise, page 220.

ing gonorrhœa, ought, in my opinion, to be differently explained. Because it appears contrary to all just inference, and reasonable analogy, to limit our ideas, and the operations of the venereal poison, by the solitary occurrence of a casualty, which only happens from one of all the numerous effects of the venereal poison.

The gland of the viper forming poison, does not, in the smallest degree, illustrate Mr. Hunter's doctrine. The structure and the susceptibilities of the vessels in the urethra, were not formed by nature to secrete poison, but those in the viper were.

In all our curative operations, that plan ought to be adopted, if possible, which will accomplish the end in the quickest and safest manner; and be attended with the least possible harm to the constitution of the patient. The effects of large and repeated doses of mercury, are not friendly to any constitution. When indispensable, unfortunately its operations are often very prejudicial, and even under some circumstances, such as cannot be admitted. It is an object well worth any surgeon's attention, to adopt that practice by which he can perform the cure with the least possible quantity of a remedy attended with danger
and

and great distress to the patient, by taking the advantage of aiding the cure by every circumstance in his power. We are not to play with the lives of our fellow creatures: though we accomplish a cure by violence, as it were, with mercury, and not quite kill our patient; yet, if we have neglected many circumstances which would have facilitated the cure, the duty which society justly demands from us, has not been performed; the surgeon acting thus is reprehensible. I sincerely hope, that Mr. Hunter's precepts on the subject alluded to, are not, at the present day, adhered to. I think it my duty to declare them dangerous.

By neglecting to remove the matter of chancre, the cure is retarded; a greater quantity of mercury will be necessary to accomplish it. By proper external applications, the cure is facilitated; the quantity of mercury that would otherwise be necessary, diminished, and the danger of contamination, and other circumstances, lessened. By allowing the matter of chancre to spread upon the adjacent parts, distressing complaints are produced; such as new chancres, inflammation, excoriation, phymosis, and paraphymosis. I have, in different instances, seen these diseases brought on by the want of proper attention of removing
the

the matter, and proper applications. These are complaints which are much better prevented than cured.

The cure of clap is not only accelerated by cleanliness, but chancre, phymosis, excoriation, &c. prevented, which cannot be doubted are objects highly worth attention.

END OF PART II.

PART III.



CHAPTER I.

EFFECTS OF THE VENEREAL POISON WHEN CIRCULATING IN THE BLOOD.

WE have previously taken notice of some of the effects of the venereal poison in its original state, and when externally applied. We now come to take a view of its effects under different circumstances ; when mixed and circulated with blood, and applied to the internal surfaces of the arteries.

The roads, the canals by which the poison is conveyed into the circulating fluid, are not at present a matter of dispute. It is transmitted from its source by the lymphatics ; the course of a great number of which have been accurately demonstrated by ancient and modern anatomists.

It

It is thought that the original poison, without producing any effect upon the part to which applied, without undergoing any change, may be received by these veins, and transmitted into the blood and produce its effects. This, I believe, to be a very rare occurrence.

I have never seen a case of lues, where I could fairly account for it upon this principle. In general, if not universally, chancre, gonorrhœa, or other local effects, precede lues. Mr. Hunter seems to have been much of the same opinion, as will be observed from the following quotation :

“ It must be allowed that this mode of absorption is very rare; and if we were to examine the parts very carefully, or inquire of the patient very strictly, probably a small chancre might be discovered to have been the cause, which I have more than once seen*.”

A longer or shorter time after the first, or new-formed poison has been received, mixed, and circulated with the blood, it exhibits itself by various effects and appearances upon the human body. It disturbs the healthy action of the heart and arteries, producing a kind of fever, attended

* Hunter's Treatise, page 256.

with restlessness, want of sleep, &c. also the healthy action of particular parts, producing a number of local complaints, as eruptions, ulcerations upon different parts of the body, tumors upon the bones, and periosteum, &c.

When the venereal poison has contaminated the blood, it exhibits itself by affecting the solid and irritable parts. The complaint is called lues venerea. Its effects now become somewhat more ambiguous, being less defined—more complicated, as interfering with a number of essential functions.

Mr. Hunter's doctrine of the lues venerea, which has gained considerable respect in the medical and chirurgical world, seems to have originated in the following manner:—

After strict attention to the different effects of the venereal poison upon different parts, when externally applied, and tracing it into the circulating blood, he expected to find that the local effects, which it then produced, would be of the precise same kind. Mr. Hunter expected to find sometimes gonorrhœa, and sometimes chancre. However, after examination, he could not find that the first of these effects were ever produced from this source; and, when its consequences were ulcerations, he found them materially different from true chancres:

chancres: therefore concludes, that the poison has lost its *specific principles*.

Mr. Hunter then, following up this idea, endeavours to prove, from observation and experiment, that the matter of those sores produced by the venereal matter, when in the circulating blood, is not poisonous.

Thus he overthrows the opinions then received, and forms the basis of a new doctrine. The manner in which the effects are produced, and the laws which govern, he endeavours to explain. These will be taken into consideration in the following pages.

In order to aid and relieve the mind, we will divide the subject into a number of distinct heads of inquiry, and, from observation and reason, deduce such distinct conclusions as we think warrantable, and afterwards connect them into one general view.

CHAPTER II.

WHETHER THE EFFECTS OF THE VENEREAL POISON WHEN
CIRCULATING IN THE BLOOD BE VENEREAL.

SCIENCE is universally undergoing changes; sometimes progressively advancing; at others, the change is retrograde. This alternate succession of rising and falling, seems to be as natural to the acquired knowledge of man, as to the individual himself.

As far as I know, until the time of Mr. Hunter, the effects of the venereal poison, when received and circulating in the blood, were thought to be venereal. Mr. Hunter's ingenuity, ever watchful, perceived a number of circumstances which appeared to him to militate against this received opinion, as we shall find from the following quotation:

“ In consequence of the blood being contami-
“ nated with real venereal pus, it might naturally
“ be supposed, that the local effects arising there-
“ from would be the same with the original which
K produced

“ produced them ; but, from observation and ex-
 “ periment I have reason to believe that this is
 “ not so.

“ In considering this subject, we may first
 “ observe, that local effects, from the constitution,
 “ are all of one species, that is ulcers, let the
 “ surface upon which they appear be what it will,
 “ whether the throat or common skin ; which is
 “ not the case in the local application of the matter
 “ in gonorrhœa and chancre ; for there I observ-
 “ ed, that it produced effects according to the
 “ nature of the surfaces. Now if the matter, when
 “ in the constitution, were to act upon the same
 “ specific principles with that which is applied,
 “ we should have gonorrhœas when it attacked
 “ a canal ; sores or chancres when it attacks
 “ other surfaces ; but it has never been yet known
 “ to produce a gonorrhœa from the constitution,
 “ though this has indeed been suspected. For
 “ some gonorrhœas, the origin of which has not
 “ been clear, and which have not easily given
 “ way to the common methods of cure, have been
 “ supposed to have arisen from the constitution.
 “ Whenever the disease affects the mouth and
 “ nose, it has always been looked upon as pro-
 “ ducing a true chancre ; yet even here I find
 “ that such ulcers in their first appearance are
 “ very different from chancres. The true chancre,
 “ I ob-

“ I observed, produces considerable inflammation,
“ which of course brings on quick suppuration,
“ attended often with a great deal of pain; but
“ the local effects, from the constitution, are slow
“ in their progress, attended with little inflamma-
“ tion, and are seldom or ever painful, except in
“ particular parts. However, this sluggishness in
“ the effects of the poison is more or less accord-
“ ing to the nature of the parts which become
“ diseased; for when the tonsils, uvula, or nose,
“ are affected, its progress is rapid, and the sores
“ have more of the chancre in their appearance
“ than when it affects the skin; yet I do not think
“ that the inflammation is so great in them as in
“ chancres that are ulcerating equally fast*.”

Admitting then with Mr. Hunter, that gonorrhœa is not caused by the poison when circulated with the blood, before we hastily make our inferences from this fact, let us take into consideration the different circumstances which are attendant, when the poison is mixed and circulated with that fluid.

Now what takes place when the poison is conveyed into the blood? The chemical changes it

* Hunter's Treatise, page 289.

may undergo, I cannot say: yet we may be allowed to observe, that in its passage into that fluid, it is mixed with the lymph of the lymphatic vessels, and poured along with it into the blood, and conveyed to the heart, where it is mixed, agitated, beat, and jumbled, with the mass of blood, and distributed to every part of the body.

After a longer or shorter time, two, three, five, or ten weeks, we have notice given of its presence, by its effects upon the system at large, and upon particular parts. Its local effects are partial, governed by laws which, at present, I am not attempting to explode. A great number of parts does not appear susceptible to its action. We find it attack, most frequently, the skin, throat, nose, coverings of the bones, and the bones themselves. Many parts of the body, as the brain, contents of the thorax, abdomen, and internal parts of generation, are not so subject, if at all, to its local effects, when mixed with the circulating fluid. The inflammation and ulceration, which it now produces, are from its contact, its application to the internal surfaces of the arteries. It seldom, if ever, when applied in this manner, produces ulceration in the urethra; we, therefore, need not be much surprized, seldom, if ever, to find gonorrhœa. This part does
not

not appear susceptible to its influence, under these circumstances.

It may not be improper here to remark, without making any conclusions respecting the question (which would be premature), that, from experience, which daily occurs, we observe the venereal poison pass from its external source into the circulating fluid, and produce particular complaints, which are known by certain characterizing marks; that these complaints are looked upon and denominated by surgeons venereal; that they seldom, if ever, disappear, but by the use of such medicines as cure the first effect of the venereal poison; that, if neglected, if those medicines be not used, almost the whole body after a time becomes tainted in a manner peculiar to such patients only who have experienced the local effects of that poison.

It then is of consequence to inquire, whether these complaints are strictly venereal, whether such as are produced, supported, and extended by the characterizing principles of the poison; and what variations are observable in its effects, depend upon other contributing circumstances: or whether it has been deprived of these characters, and now operates as another cause.

As we only become acquainted with the nature of the poison by its effects upon the human body, the sole way of determining this inquiry with justice is, accurately to examine those effects when exhibited to us, and accordingly make our inferences.

The poison affects various parts, after having been received into and circulated with the blood. If the throat be attacked by it, an ulcer is formed singularly marked—a deadened, whitish, sloughing bottom—hardened basis—edges abrupt and irregular—a considerable inflammation appears around—a product of matter issues from the sore, and the disease extends often very rapidly. All these symptoms are minor ones of chancre, which forms on the inside of the prepuce.

If a blotch appear upon the dry hard skin, as the arm or breast, the cuticle becomes diseased, and often presents an appearance like bran, which, if rubbed or thrown off by the regular process of the disease, is again replaced by those which gradually become more crusty, and at last a scab. These appearances gradually enlarge and extend, and an ulcer forms.—These preparatory appearances, and even the ulcer itself, are also minor ones of those presented, when chancre appears on the body of the penis.

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The ulcer thus formed, slowly extends, without recourse be had to mercury, when they generally disappear: therefore, as far as these observations extend, they authorize us to infer, that they are venereal, of a nature similar to those of chancre.

CHAPTER III.

WHETHER THE PRODUCT OF SORES IN LUES VENEREA
BE POISONOUS.

INGENUITY is not, perhaps, unfrequently connected with a great desire for novelty. One innovation generally requires a number of others, which in their turn have similar demands.

One distinguishing characteristic of venereal sores is, that the matter issuing therefrom is poisonous. Now, if the poison, when circulating in the blood, loses its specific or characterizing principles, the product of these ulcers will not be venereal; and this Mr. Hunter asserts, which he endeavours to prove from observation and experiment. Coming from such authority, and so supported, the idea requires great consideration.

“ When the matter has affected the constitution,
 “ it from thence produces many local effects
 “ on different parts of the body, which are in
 “ general a kind of inflammation, or at least an
 “ in-

“ increased action occasioning a suppuration of
“ its own kind. It is supposed, that the matter,
“ produced in consequence of these inflammations,
“ similar to the matter of a gonorrhœa or chancre,
“ is also venereal and poisonous.

“ This I believe till now has never been denied;
“ and, upon the first view of the subject, one
“ would be inclined to suppose that it really should
“ be venereal: for first the venereal matter is the
“ cause; and again the same treatment cures both
“ diseases; thus mercury cures both a chancre
“ and a lues venerea; however this is no decisive
“ proof, as mercury cures many diseases besides
“ the venereal. On the other hand there are
“ many strong reasons for believing that the mat-
“ ter is not venereal. There is one curious fact,
“ which shows it is either not venereal, or if it
“ be, that it is not capable of acting in some re-
“ spects on the same body or same state of con-
“ stitution as that matter does, which is produced
“ from a chancre or gonorrhœa. The pus from
“ these latter, when absorbed, generally produces a
“ bubo, as has been described; but we never find
“ a bubo from the absorption of matter from a
“ pocky sore; for instance, when there is a vene-
“ real ulcer in the throat, we have no buboes in
“ the glands of the neck; when there are venereal
“ sores on the arms, or even suppurating nodes
“ on

“ on the ulna, there are no swellings of the glands
“ of the arm-pit; although such will take place
“ if fresh venereal matter is applied to a common
“ sore on the arm, hand, or fingers. No swelling
“ takes place in the glands of the groin from
“ either nodes or blotches on the legs or thighs.
“ It may be supposed that there is no absorption
“ from such sores; but I think we have no grounds
“ for such supposition. Its mode of irritation, or
“ the action of the part affected is very different
“ from what happens in the chancre, gonorrhœa
“ or bubo, being hardly attended with inflamma-
“ tion, which in them is generally violent*.”

Mr. Bell, with whom I have already taken liberties, and to whose labours the surgical world is much indebted, has taken into consideration this opinion, and potently observes—

“ I cannot say what may have been the result
“ of Mr. Hunter’s experience, but I have met
“ with many instances of buboes, or swelled glands,
“ in the thigh, axilla, and neck, as obviously pro-
“ duced by sores in the feet, legs, hands, and
“ throat, as buboes commonly are in the groin
“ from chancres on the penis. But admitting
“ that the occurrence is not frequent, still this is

* Hunter’s Treatise, page 292.

“ no reason for supposing that the matter of these
“ sores is not venereal, &c.*”

If I can believe the evidences of my own senses, I have, in different instances, seen bubo take place in a lymphatic gland, from the reception of the matter of a venereal or pocky sore. I have seen buboes in the lymphatic glands, under the lower jaw, from ulcers in the contiguous parts, as the prolabium of the upper lip, the prolabium of the lower lip, and the tongue and inside of the lower lip. I have seen a bubo in the arm-pit, from a venereal sore on the middle of the arm; and one a little above the elbow, from an ulcer on the fore arm, situated rather between the outer and inner part.

In all these cases, the ulcers themselves had the characters of venereal ulcers, and all of them were attended with venereal eruptions upon different parts of the body. They all declined and got well by a course of mercury.

From the situation of these venereal sores, from the tumors forming in lymphatic glands, into and through which, the lymph of the lymphatic vessels of the respective parts run; and, from their

disappearing by the effects of mercury, I think I may be allowed to say, they were venereal buboes, caused by the matter of these sores being received by the lymphatics, and by them conveyed into these glands; therefore, the matter thus occasioned, retains the poisonous properties of chancre. But as this is not so frequent an occurrence, as from the immediate reception of the matter of chancre, we are obliged to admit that it is somewhat enfeebled: its virulence is not in so high a degree as that of chancre.

The matter of those sores, produced by the venereal poison circulating with the blood, will not only produce bubo in the same person; but, when locally applied to an untainted one, will produce chancre, and all its consequences. I will recite a case which lately came under my own observation.

About six months ago, a young lady came under my care, for the cure of a venereal eruption upon her back, and venereal ulcers upon the velum pendulum palati, and upon the prolabium of the lower lip. She had chancres some months before, which were cured by mercury. At this time, about six months from the present, a young gentleman, who visited her, indulged freely in saluting her, who was totally ignorant of her
com-

complaint. A chancre took place on the prolabium of his upper lip, which was disregarded for nearly a month, when a large swelling appeared in a lymphatic gland, under the lower jaw, on the right side, which was soon followed by copper-coloured eruptions upon the breast and arms. In this state, he applied to me for assistance. The symptoms all vanished by a course of mercury. He was a very regular young man, and, according to his word, never had any venereal complaint before.

In this instance, I say, the complaint of the young gentleman was venereal, because the chancre on the lip and blotches upon the skin, which disappeared by mercury, characterized it; that he was contaminated by her, because no other source could be suspected; and its appearing first upon the lip, next infecting a lymphatic gland under the jaw, and afterwards the skin, I think, warrants the conclusion. Therefore, I would say, that the matter of a pocky sore is poisonous, and will not only produce its effects upon a system already contaminated; but also will excite both local and general effects upon a sound body.

Mr. Hunter then proceeds to prove, from experiment, and inferences deduced from a dreadful, but still ambiguous case, that the matter of venereal sores has not the same quality as that of chancre. By inoculating contaminated persons with the matter of their own sores, and that of chancre and gonorrhœa, he found he only produced the venereal effect by that of chancre and gonorrhœa, therefore concludes that the matter of venereal sores is not infectious.

Mr. Clutterbuck has made an ingenious scrutiny of one of Mr. Hunter's experiments, and think it does not warrant so comprehensive a conclusion. He also judiciously remarks, "Had
" the matter from the mother been applied to a
" sound person, instead of herself, the experiment
" would have been more conclusive*."

The experiments of Mr. Hunter are, in more respects than one, objectionable. In instances which come daily under our observation, and represented to us by the powers of the system, ought we not rather to depend upon these than an experiment? Human experiments are often full of error, even when conducted by the most careful and discerning mind, and performed by the hands of the most skilful experimenters. Indeed, nu-

* Mr. Clutterbuck's Letter, page 37.

merous operations of nature are by us inimitable. —We cannot see the extent of her means; even the effects of our own experiments often delude our senses.

Let us not deceive ourselves.—The experiments of Mr. Hunter do not represent exactly what takes place in the system. *Lues venerea* (which, I presume, no person will doubt to be an effect of the venereal poison) is produced by the application of the poison to the internal surface of the vessels. Mr. Hunter's experiments of inoculation, mechanically injure, and bring the poison to the external surfaces and ends of those injured vessels; therefore, if no local effect were by this means produced, it does not evince to us the entire want of virulence in the matter: May not the mechanical injury, in some measure, prevent the effects, by its immediately producing inflammation, and thereby rendering the vessels unsusceptible to its power? According to Mr. Hunter, he often failed to produce the venereal complaint by the application of even the matter of chancre to a common sore. Certainly, the chance of producing it, appears much smaller, when a part is only punctured with a lancet dipped into the matter of a venereal sore.

If any dependence can be placed upon human reason, guided by the aid of limited facts, we
have

have the strongest support for these opinions. We know the poisonous effects of the matter of chancre, when locally applied to the external surface of irritable parts; that its product is venereal; and if it be received into the circulating fluid, and there produce any effects, these effects, we might expect, would be venereal, for it then acts upon parts possessing the same irritability, which act in the same manner. And we know of no other effects of the venereal poison, but those which are styled venereal.

We also may derive considerable support from analogy. In the instance of small-pox, the virus produces local effects; the original, or the new-formed matter, may be received into the circulating fluid, and produce the pustules, the product of which is poisonous: This, at least, proves that such circumstances are not contrary to what may take place in the animal œconomy.

We may, therefore, conclude, from careful observation, that the venereal poison, when received into the circulation, and by this means applied or brought in contact to the internal surfaces of irritable living arteries, produces the venereal disease; that, in general, if not always, when so applied, its effects are inflammation
and

and ulceration; that the action thus exerted, is upon the contaminated fluid which they are now circulating; and that the product so elaborated, is venereal. The poison, then, does not lose its particular or poisonous characters, by its mixing and circulating with the blood—the disease and product are still venereal.

CHAPTER IV.

WHETHER THE POISON CONTINUES TO CIRCULATE IN
THE BLOOD, OR IS SOON THROWN OUT.

THERE are some *causes* which we cannot make visible to our sight: we can only infer their existence from their perceived effects. This circumstance perpetually occurs in natural philosophy. It is not, therefore, a decisive evidence against the existence of a cause asserted to be acting, that we cannot visibly shew it. If, from a careful investigation and comparison of effects, we can with just reasoning infer its existence, we do all that we can effect in a state of imperfect knowledge. In every acid, we infer the presence and action of that acidifying element which we call oxigene, though human science has not yet been able to view this agent in any separate state.

When we observe certain local effects upon the body, we infer that venereal poison has been conveyed into the circulating fluid; we infer
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the presence and action of that agent; but we cannot, in our present state of knowledge, pretend to give visible evidence for our assertion. In this, and in many other instances, our belief originates from probable conjecture. After repeated observations of particular symptoms of disease (be it chancre), and that the person thus affected, is liable to other particular complaints, as eruptions upon different parts of the skin, ulceration in the throat, &c. we pronounce, from the aspect of these effects, that the matter of chancre conveyed from that source by the lymphatics is, or has been circulating with the blood; but we cannot give a visible demonstration of our assertion. It is represented to us by its effects: these effects being such, as no other cause which we are acquainted with produces, our inference is warrantable.

After having traced the venereal poison into the circulating fluid, and taken into consideration some of its effects upon the solid irritable parts, the questions, what afterwards becomes of it, and whether it continues to float along with that vehicle, or passes out of it along with the product of those effects, and the natural secretions of the body, immediately present themselves.

It appears, that the prevailing opinion, which maintained its respect for a considerable time, was, that the poison, when once admitted into the blood, had the power of contaminating the whole mass of blood; that the waste and renovation, which are perpetually going on in that fluid, had not the power of expelling it; that it only was annihilated by the force of medicine.

Mr. Hunter, from his particular train of reasoning—from his peculiar manner of viewing the operations of the human body, was led to a different conclusion. He asserts, that the poison “only irritates when just absorbed, and is soon expelled or thrown out in some of the secretions*.”

This, and such like questions, are no less curious, difficult, and important. If we have any rational mode of practice, it must be governed by the decision of such questions; and, if these are disregarded as theoretical, our practice will be at random; it can only be right by chance, and we not know the reason why.

Mr. Hunter, finding that different parts of the body became diseased, somewhat successively, ac-

* Hunter's Treatise, page 314.

counted for it, by supposing different degrees of susceptibility in different parts, and divides them into *first in order*, and *second in order*. Under the *first*, he includes the skin, tonsils, nose, throat, inside of the mouth, and tongue; under the *second*, the periosteum, fasciæ, and bones*.

Confiding in these opinions of Mr. Hunter, and finding that different parts susceptible, do not for a long time after the reception of the poison, become diseased, a chasm is made in our reasoning; because, one would be led to expect that all those parts susceptible to the effect of the venereal poison, would soon become affected. Mr. Hunter fills up this gap, by supposing that, during the time between the reception of the poison and its visible effects, a *disposition* is formed, or forming, in that part which “must necessarily go on to form “the venereal action,” because “mercury does “not destroy a disposition already formed †.”

Again, upon the truth of these opinions depends another incident: for it is essential to them that a part affected with the poison, coming to it through the medium of the circulation, once cured, cannot again become diseased from the same source, which Mr. Hunter affirms. “A part

* Hunter's Treatise, p. 306. † Hunter's Treatise, p. 315.

“ once perfectly cured is never irritated again by
“ the same stock of infection ||.”

Mr. Hunter's singular opinions do not end here; he proceeds to deny, that a child in utero can be contaminated, either from the mother or father*.

I cannot agree with Mr. Hunter in these opinions. If any alteration has taken place in the parts, which he has pronounced to be *disposed*, and by the local contact of the poison, then it is disease, and venereal: If the poison has *not* altered the arterial action, or physical structure of the part, in that case, there is no disease: and, as to the *disposition* of any part of our body to a particular disease, this must of course be a mere metaphor, because a part, which has no mind, cannot act, but from the occurrence of a physical cause.

If Mr. Hunter's opinion were true, that all the parts susceptible to the effect of the venereal poison, when in the circulation, were contaminated at once, and must necessarily come into disease; should we not find the venereal disease much more inveterate than what experience informs us of?

|| Hunter's Treatise, p. 314. * Hunter's Treatise, p. 291.

If the skin became diseased, would not the throat, periosteum, bones, &c. certainly follow? But we frequently, indeed, find the tonsils ulcerated from the venereal cause, and no other part. If this effect be cured by mercury, and that medicine be continued for a proper length of time, in general, no other part becomes diseased. How could this happen, agreeable to Mr. Hunter's opinion? If any alteration had taken place from the influence of the poison, which Mr. Hunter would assert to be in a state of disposition, that alteration must have been venereal; and, being venereal, mercury would have cured it, because every person is aware that a small effect is more easily cured than a large one.

To make his opinion a little more consistent, it was necessary for him to proceed in the same track a step further; for, if the poison was soon expelled out of the system, then when a part was once cured, it would not again become affected without a recent contagion, which Mr. Hunter asserts. How far this opinion is sanctioned by careful observation, is worth inquiry!

Mr. Clutterbuck has mentioned, in his letter, a number of cases proving the contrary. From my own experience, I think, that it is no uncommon

mon circumstance for the disease to return without a fresh infection.

I have seen the disease return to its former seat in a variety of instances, without any fresh infection, as far as appearances and evidence could indicate.

We frequently find that a person who has had, or who has, a chancre or gonorrhœa, suffers certain derangements in his general health, as a kind of fever, attended with head-ach, restlessness, want of sleep, pains in the limbs, &c. These symptoms are succeeded by different local complaints, appearing upon different parts, and in an uncertain series of order. These complaints are eruptions upon different parts of the skin, ulcerations in the throat, enlargement of the periosteum and bones, &c.

Now, what takes place, appears to be this;—the venereal poison has been received into and mixed with the circulating fluid, and conveyed along with it to all the different parts of the body: hence its effects are remote and wide upon those parts which are susceptible to its influence. They are remote from the source from which the poison sprung—remote from the center, and principal

cial source of the circulation—remote respecting their local situation to each other.

Its effects are now produced, by the poison being circulated with the blood, and transmitted from the large into the small arteries. By its application, or contact, with the internal surface of the arterial system, it disturbs the healthy action of the heart and large arteries, producing the fever; and, in the smaller ones, it produces inflammation, which often goes on to suppuration and ulceration.

We also find from experience, that different parts become diseased, one after another, from the same infection. If a venereal sore throat be disregarded, in the course of a longer or shorter period, the nose, skin, and periosteum, often become affected successively. If one of these local effects be cured by mercury, and that remedy be immediately discontinued, often, after a long or shorter time, the disease returns to the same part, or perhaps affects a different one, and then returns to its former seat. This event, which is denied by Mr. Hunter, I will illustrate by a few facts, from one of the most miserable cases of the kind, which it was my misfortune to witness for more than two years.

In November 1796, I attended a young delicate gentleman, for the cure of a small chancre upon the prepuce. Being ignorant respecting the complaint, he had allowed it to go on uninterrupted for about four weeks. He had also a venereal ulcer upon the velum pendulum palati, near the root of the uvula, and copper-coloured eruptions upon the neck and breast, and scabby ones upon the right ear. By the use of mercury, these complaints all disappeared in about a month; and, contrary to my express desire, he soon after laid aside the use of mercury.

The latter end of January, 1797, he was again seized with ulcer upon the arch of the velum pendulum palati, scabby eruptions upon both ears, and eruptions upon the breast, without any fresh intercourse or disease upon the parts of generation. He now bore the mercury extremely ill, and the symptoms were not removed until about the fourth of April. About the twentieth of this month, he became extremely ill of a fever, which obliged him to desist from the use of mercury for three weeks. He was now unwilling to have recourse to it again, dreading its effects from experience.

In about six weeks, the eruption upon the breast, and ulcer upon the velum pendulum palati,

lati, again returned. He now took double the quantity of mercury to make his mouth sore: the symptoms did not totally disappear till November. During this time, I was obliged to discontinue the mercury several times, on account of its producing violent febrile symptoms, accompanied with great general weakness, loss of appetite, &c. notwithstanding the liberal use of bark, cool nourishing diet, &c. &c. The mercury was continued, in as large quantities as the circumstances would permit of, for near six weeks, its use being occasionally interrupted, during this period, two or three different times.

I now expected that the complaint had been completely conquered; but, to my great surprise and sorrow, February, 1798, I found enlargement of the lower extremity, and outer surface of the radius of the left arm, an ulcer on the thick part of the fore-arm, and a scabby eruption upon the back. Mercury was now had recourse to, and small quantities had great effects upon the mouth and salivary glands. The distresses it occasioned were great, whether given internally or externally, and with whatever medicine united. Every now and then the action of the heart and arteries were so fretted, as to oblige me to omit it, until those effects were removed, when the complaint

plaint became stationary, or rather increased. However, after six month's hard working, great patience and attention on the part of my patient, the symptoms were totally subdued in August. The mercury was still continued (though not in so large quantities, nor so regular as could be wished, on account of the distressing symptoms it excited) for several weeks. Notwithstanding, in the beginning of December, the enlargement of the bone, and eruption upon the back, again appeared, and also the scabby eruptions upon both ears. Mercury was again had recourse to, and continued, with occasional intermissions, till May 1799, when the symptoms all disappeared. Mercury had now a powerful effect upon the habit, and upon the mouth; half a grain doses produced as much effect upon the mouth, as grain ones did in the first instance. Towards the conclusion, these small quantities of mercury had a happy effect upon the complaint, and his health surprisingly recovered. I had the great pleasure of seeing him get lusty, and he now remains well.

It will be perceived, that the cure of this case was retarded, and, probably, its inveteracy increased, by the peculiarity of the constitution. The distressing complaints occasioned by the mercury, rendered it inadmissible in sufficient quantities

tities, and for a time long enough to complete the cure.

It would then appear that the venereal poison, when once received into the circulating blood, is not soon thrown out, but continues to float with that medium for an unlimited time.

I am aware, that I may be told, that the blood is perpetually undergoing changes by waste and renovation; so that the blood, which now circulates in the body, is not that same blood which circulated some little time back; and that the poison must soon be thrown out of the system along with the secretions.

This may appear consonant with physiological reasoning, if we disregard those facts which are so frequently represented.—But, if we find different parts become diseased successively, and those parts get well and again become affected, and from the same infection, must we not still infer the presence of the poison? If not, we are compelled to imagine, that those parts again become diseased without the occurrence of the same cause, which is highly improbable.

These effects may arise from a source, which ought also to be taken into account. This is, that
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the matter of venereal sores being poisonous, may be again taken into the lymphatics, and conveyed into the blood, and may produce the same effects. But it would appear that, independently of this supply of venereal poison, the poison first received into the blood, has some power of supplying the waste, which must necessarily happen; for we often find the venereal poison remain inactive in the system for a long time. Also, the complaint returns to the skin, or bones, a considerable time after the previous one had been removed; the first complaint having been cured, before it had advanced so far as to form matter. These, and similar instances, I conceive, warrant us to conclude, that the poison originally conveyed into the mass of blood, has a power, whilst in that fluid, of supplying the waste it may necessarily be subjected to. How this is accomplished, will be considered in the following chapter.

As to the child in the womb being affected, so many instances may be collected to prove it, that it would be scrupulous in the extreme to deny them, especially from the mother. Though, in our present state of imperfect knowledge respecting impregnation, we cannot clearly explain how this fact does take place, yet, when it is presented, we are not to disregard it.

I believe, no connection between mother and child, by continuity of vascular canal, has yet been demonstrated. The umbilical arteries and vein certainly anastomose, because injections will pass easy from the one into the other: therefore, though we cannot say, that the child receives any of its mother's blood; yet the vessels of the mother and child meet in the placenta. From this fact, from what has been observed to transpire in the venereal disease, and in the small-pox, we must admit an indirect communication. The manner of this, I will not attempt to conjecture here: time and industry, I trust, will truly unfold it at some future period.

Can we conceive the child to be contaminated from the ovum being so at the time of impregnation?

How the child becomes contaminated from the male, the mother being untainted, is still more difficult to account for; because, allowing the semen to be contaminated with venereal poison, how does it come in contact with the impregnated ovum?

Agreeable to the accurate anatomical observations of Dr. Marshal, the womb is firmly closed, and therefore it seems to follow, that the semen
cannot

cannot enter. If so, it cannot come in contact with the ovum. These observations are further confirmed by the experiments of Harvey; and they incline me to doubt, that though the semen should be contaminated, the child would be so too, without disease in the mother.

The opinions and facts of Mr. Bell, on this point, merit some attention. He mentions a circumstance, which seems to confute my opinion*.

* Bell's Treatise, vol. ii. page 420.

CHAPTER V.

WHETHER THE POISON INCREASES IN THE BLOOD.

OUR next consideration will be, whether the effects so widely dispersed, and appearing at such different periods, and occasioned by the poison being circulated in the blood, be all produced by the quantity of matter, or poison, received from the first local complaint; or, that the poison by some means has received a supply after being taken into the blood.

When the venereal complaint became prevalent; when the professors of physic could trace the poison from the external surface of the body, by the appropriate canals, into the circulating blood; when they found that it then affected so many parts widely distant from each other, and disordered many of the functions; and when they began to employ some rational physiology on those effects and consequences, they found themselves not able to account for them, without admitting the poison to be increased in that fluid. For, having followed the poison into the subclavian

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veins,

veins, and having there found it mixing with the blood, they reasoned somewhat as follows :

The poison being perfectly mixed with the blood, by the systole of the heart and arteries, cannot produce so many and so widely distant effects, without being first increased or multiplied*.

Mr. Hunter seems to have disregarded this kind of reasoning, as void of foundation, and thinks that all those effects of lues proceed from the quantity of poison received from chancre or gonorrhœa.

In order to come at a proper decision upon this question, we must consult the disease—we must observe and trace the series of effects, and draw such conclusions as they dictate. We are not supporting nor opposing ; we are seeking for truth, of which experience provides the evidence. The facts presented to us, may be overlooked, but they cannot be altered.

We may often observe, a long time after a small chancre or gonorrhœa has been healed (perhaps several months), that the patient becomes singularly affected with dubious symptoms of fever, much fretted pulse, great restlessness, peculiar sensations

* Astruc's Treatise.

and pains in the pericranium and bones of the head, gnawing pains in the bones, disturbed sleep, irregular appetite, and troubled countenance. These symptoms foretel some impending, but still ambiguous mischief. Time, an increase of these symptoms, and the appearance of new ones, unfold the mystery.

Perhaps, a great part of the body becomes covered with peculiar eruptions, one or more of the bones enlarged, and the throat ulcerated. The preceding ambiguous symptoms are irregular—they sometimes precede, sometimes perhaps, tho' rarely, follow the less dubious and local ones. These effects go on increasing; and, if left to themselves, the whole body appears one tainted mass, and ultimately the individual is destroyed.

When the symptoms have been removed by the force of medicine, and the patient has remained well for six, eight, or a greater number of months, the disease may again return, and the same or different parts may be again affected. All the symptoms may appear about the same time, or in succession, the succession being in a varying order, and after a longer or shorter period.

Now are so many functions disturbed, and so many local complaints produced from the original

quantity of the poison received from the chancre or gonorrhœa? Or, has the poison been supplied or increased when in the blood?

At first view, the opinion, that all those complaints are immediate consequences of that quantity of poison taken into the blood from the chancre or gonorrhœa, appears extravagant; and, as the blood is perpetually wasting by its supplying the materials out of which the numerous secretions are formed, and that want constantly replenishing by the received chyle and lymph, were there no means by which the poison accumulates, that stock would most likely be soon exhausted by the perpetual loss. But, as I have said, we find those effects produced some months after the time of the reception of the poison into the circulating fluid.

Does it not also appear extravagant to expect, that the action of the heart and arteries could be so much disturbed from a few drops of matter of gonorrhœa or chancre, distributed and mixed with such an abundant fluid as the blood? And, if these effects were all produced from this comparatively small quantity, would they not appear before the space of six, eight, or ten months? During that time, the blood has been often thrown out in the numerous secretions of the system, and recruited again:

again: of course the blood which received the poisonous drop, must have passed away long before the venereal effects appear. What renders the poison inert for so long a time? And what enables it to act after the blood has been so often changed? Certainly, this cannot be accounted for upon the ground of susceptibility; for, though the quality of admitting disease is very various; yet, without a supply or increase, the poison would certainly be expelled, or thrown out of the system along with the secretions, long before the specified time.

I am apprehensive, that the theorists of the present time will object to that affection of the heart and arteries being caused by the state of the circulating fluid; that it will be represented as proceeding from the local effects upon the solids.

It often happens, that opinions gain admittance into the medical world, and prevail for a length of time, and, perhaps, for no other reason than the fame of their authors or adopters. The rage for explaining all the phenomena of disease, by a supposed vitiated state of the fluids of the body, had its day, and gave place to a rising one, which now maintains as much credit as its predecessor. The fate of this, though placed

upon the less changeable parts of the body, namely the solids, will, probably, be similar after a longer or shorter period, and whether established upon truth or falshood.—Truth may be a more permanent basis than falshood; but epochs have appeared, when it has been overpowered by chimeras or hypothesis.

At the present day, the incidents attending lues venerea are pretty generally attributed to some local injury, or affection, of the solid parts; and the functions of others, remote from the local disease, are supposed to become diseased in no other way, and, in a secondary manner, by sympathy.

This kind of philosophy is not limited to the venereal disease: I believe, it extends its influence to others. This does not concern my present purpose; but it is indispensibly necessary to take some notice of it, so far as the venereal disease is implicated.

Mr. Hunter, and his advocates, are compelled to espouse this opinion, because we often find fever in lues venerea appear long after the venereal poison (according to their mode of reasoning) is thrown out of the system; therefore, to admit this fever to be the immediate consequence of the
poison

poison floating in the circulating blood, would be to destroy their own opinions.

Without making any comment upon the word sympathy, which is a metaphor that can hardly be used in science with impunity; or even without searching for inferences to acquire the author's meaning, when he uses the expression; it would appear plain, that he does not consider that affection of the arterial system, attending lues, and denominated fever, as an immediate consequence of the venereal poison, but as a secondary one, produced by some local effect of that poison. "The hectic fever is an universal sympathy with a local disease, which the constitution is not able to overcome*."

Respecting this important question, Mr. Hunter, in another part of his work, betrays his doubt; for he observes, "When the parts are contaminated by venereal poison, we commonly find fever, restlessness, or want of sleep, and often head-ach; but I believe that these symptoms are rather peculiar to the disease, when the second order of parts, the periosteum and bones, are affected, although they are sometimes found of the first. Do these symptoms arise from the

* Hunter's Treatise, page 1, Introduction.

“ local irritations affecting the constitution? And
 “ are they merely sympathetic*?” These ques-
 tions, put by Mr. Hunter, he leaves unanswered.

In the next paragraph, he observes, “ These
 “ symptoms often take place independent of, and
 “ unattended by, any local action; and when that
 “ is the case it becomes very uncertain what the
 “ disease is; for in cases, not admitting of clear
 “ proof, we must rest on the concurrence of other
 “ circumstances. Many of these symptoms give
 “ way to mercury. This is probably the only
 “ concurring circumstance attending this com-
 “ plaint that is any proof of its being venereal.
 “ It rather, however, appears to militate against
 “ this idea, that, for the most part, a much
 “ smaller quantity is sufficient for the cure of
 “ such symptoms than what is necessary for the
 “ cure of local complaints.” Thus he leaves in
 doubt this important question, upon a decision of
 which, depends many points of his doctrine.

If the function of the heart and arteries in lues
 venerea, only become diseased in consequence of
 the disturbance given them by the local mischief
 of the venereal poison, the fact ought to be clearly
 exhibited.—If they be disturbed by the presence
 of the venereal poison in the circulating fluid, for

* Hunter's Treatise, page 300.

the interest of individuals it should be taken into account.

Will it not be admitted, that if the fever in lues proceeds from the local injury of the venereal poison, the fever so produced would universally follow? Or only appear when those local effects were present? Is it not essential for the cause to precede the consequences? I presume these positions will be admitted; therefore, if the fever goes before the local effects (I mean the local effects proceeding from the blood being contaminated), the question, I conceive, is decided.

If we consult our own experience, or even have recourse to the writings of Mr. Hunter, we shall find the evidence in favour of the first appearance of the fever. "These symptoms" meaning the fever, &c. "continue for some days, and
" often for weeks, show that there is some irritating cause which works slowly upon the constitution. It is then supposed to be whatever
" the invention or ingenuity of the practitioner shall call it; but the venereal eruptions or nodes
" upon either the periosteum, bones, or tendons,
" or other parts, appearing, show the cause, and
" in some degree carry off the symptoms of fever
" and relieve the constitution for a little time,
" but they soon recur*."

* Hunter's Treatise 130.

Now, if the venereal eruption, &c. remove the fever, with what justness can we suspect them to be the cause? Can we suppose the cause to relieve what it necessarily produces?

If we consult our own experience, I am persuaded, we shall very often find the action of the heart and arteries become disordered; restlessness, head-ach, &c. followed by the local effects of the poison; therefore, with what justness can we attribute these preceding complaints to those which succeed? The speculation appears unwarrantable. It is reversing the order of nature, by assuming data agreeable to our own imagination, not deducing such inferences as the facts themselves point out.

Now, if we cannot, from reasonable analogy, infer the numerous effects of lues to be occasioned by that quantity supposed to be received from the chancre or gonorrhœa, two other means may be pointed out; and, first—

We often find that the effects in lues follow one another; perhaps an ulcer appears on the tonsils, which will be succeeded by blotches upon the skin, and these again succeeded by enlargements of the bones, &c. Now, if we have rightly inferred, that the matter of these local effects is
poisonous,

poisonous, may we not be allowed to attribute some of these succeeding effects to this source? We have no reason to doubt (at least any that I am acquainted with), that this matter is taken into the lymphatics and conveyed into the blood. Therefore we may reasonably infer this to be one source; because, if it be poisonous, and be again received by the lymphatics and conveyed into the blood, it will produce poisonous effects.

But, I think, we cannot by this sufficiently account for all the phænomena of this disease; because, as before observed, we sometimes find the complaint break out in a variety of parts, nearly about the same time, the origin of which was only a small chancre or a gonorrhœa, which lasted but a few days. Also, the poison may remain in the blood for a number of months, before it produces any visible effects upon the irritable parts. In this last instance, had not the poison itself some power of increasing, and by that means retaining its situation in the blood, it would appear, that it must have been thrown out of the system, along with some of the secretions before that time.

How this increase happens, is also a matter worth consideration. It has been thought, that this effect was accomplished by a kind of fermentation taking place, when the poison was mixed
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in the blood, by which the blood was converted into venereal poison. This theory, however, is liable to numerous objections. However, I am somewhat of opinion with Mr. Bell, who observes, “ I think it highly probable that the matter of
 “ all contagious diseases, and more especially the
 “ matter of lues venerea, has a power of assim-
 “ lating to its own nature a certain portion, and
 “ ultimately, perhaps, the whole fluids of the
 “ body*.”

At least, I am of opinion, that it has the power of converting or assimilating the *blood*, or *some* of its constituent principles, into venereal poison.

These observations will acquire illustration, by a short quotation from Mr. Bell's Treatise. “ From
 “ all this I conceive that in lues venerea the
 “ matter of contagion acts in the first instance
 “ upon the fluids, and that the solids are not
 “ otherwise affected, than in being excited by this
 “ morbid state of the fluids &c.” And, again, in the next paragraph, “ From this view of the
 “ origin of lues venerea we account for the mat-
 “ ter of contagion remaining longer inactive in
 “ some instances than in others. Besides any dif-
 “ ference which may take place in the state of the

* Bell's Treatise, vol. ii. page 164.

“ fluids upon which the matter has to act, and
“ by which the assimilation to which we allude
“ may be accelerated or retarded, the different
“ degrees of irritability which prevails in different
“ persons, and in the same person at different
“ times, will also co-operate, &c.” vol. ii. p. 165.

Perhaps, the strongest analogical support, which these observations receive, is from the small-pox. We find that the smallest particle of this matter, inserted between the skin and cuticle, produces inflammation and suppuration. It is generally received into the circulating blood, from which the whole surface of the body may become covered with pustules. The matter of each of these is of the same nature, and possesses similar properties to that applied between the skin and cuticle. Would any person here say, that the particle inserted, or what quantity of the produce, which might be received from this local effect, was conveyed into the blood, mixed with it, and circulated to every part of the body, and produced this universal effect, without being first strengthened or increased in that fluid? Is not the blood, at least, the vehicle through which the matter produces the effect; but can we suppose that such a small imaginary quantity could affect so generally? It is highly improbable; nay, I had almost said, impossible. It must have been increased in quantity,

tity, or its effects could not be so general. The same may be said of the venereal poison.

The cause, then, of each of these complaints is in the blood: It acts upon that blood first; then upon the solid irritable fibre, when its effects become visible. The blood is not the mere vehicle; it does more in this great business. It (or some of its constituent principles) affords a convertible principle to the poison, and becomes poisonous. But, by what process this is accomplished, I cannot say. I only infer the effect, not the means.

If, then, the blood, or some of its constituent parts, is contaminated, or chemically converted by the poison, when circulated along with that fluid, is it not reasonable to expect, that the secretions elaborated from that contaminated fluid may be tainted? But, at the same time, it is also reasonable to suppose, that they may be only slightly so, and, in many instances, so slightly so, as not to be able to communicate the disease. However, I will not further insist upon this circumstance, because, though I have met with one or two circumstances, which seems to strengthen this reasoning, yet I have not collected sufficient evidence to confirm it.

CHAPTER VI.

HOW THE PRODUCT OF SORES, IN LUES VENEREA,
BECOMES POISONOUS.

IF the fluid, from which the pus is elaborated, be contaminated, is it not reasonable to suppose, that the pus itself will be so too? We could hardly expect a wholesome product from tainted ingredients.

The effects in lues, I have observed, are produced by the venereal poison floating in the blood, or from contaminated blood. It is brought in contact with the internal surface of the arteries, and thereby produces the venereal inflammation. The venereal inflammation advances to suppuration, the softened and deadened parts undergo a retrograde process, and the living inflammatory acting vessels form matter. The arterial effect, which elaborates the product, being upon the tainted fluid which they circulate, is venereal or poisonous, and even now, as it flows from those arteries which form it. But now, though the product of those

those arteries be poisonous as it leaves the vessels, it would not appear to be so necessarily, from that particular arterial action which produced it; but because the ingredients, from which it is taken, were contaminated.

The poison is now composed of the decomposed solid particles of the part, with their juices, and the tainted product of the venereal inflammation.

CHAPTER VII.

WHETHER THE EFFECTS OF THE VENEREAL POISON
BE LIMITED.

MR. HUNTER, in page 304, speaks of the venereal inflammation being confined within what he calls a specific distance.

“ I have already observed, that many specific
 “ diseases, as also those arising from poisons, have
 “ their local effects confined to certain distances,
 “ which I have called their local specific distance;
 “ and it would appear from observation, that the
 “ venereal irritation and inflammation, of whatever
 “ kind it may be, is guided by this principle;
 “ for it seldom extends far beyond the surface
 “ that receives it; the neighbouring parts not
 “ having a tendency to sympathise, or run easily
 “ into this kind of inflammation. This is the
 “ reason why we find a gonorrhœa for weeks
 “ confined to one spot in the urethra in men, and
 “ for months to the vagina in women, not extend-
 “ ing further in either. In chancre also the in-
 “ flammation is confined to the seat of the sore

N

“ with-

“ without becoming so diffused as when from
“ common accidents, &c.”

I think, upon faithful observation, it will appear, that nature has not fortified the human body with any laws to stop or limit the progress of the venereal disease.

It appears, that the venereal poison, when brought in contact with the human body, produces a kind of inflammation, which generally runs into ulceration; that the ulceration is a dissolution, or decomposition of the solid substance of the part; and that this process, when begun by the venereal cause, spreads around to the neighbouring parts.

Now, that the poison caused the disease, will, I apprehend, be readily admitted. But I have also asserted, contrary to the opinion of Mr. Hunter, that the continuation and extension of the venereal disease are caused, *and* by the effects of the poison.

As the ulceration advances or extends, the poison is brought into contact with fresh parts, the disease is communicated to those parts, and then goes on progressively, without knowing any bounds.

Cor-

Correspondent to this we find, that if the effect of the venereal poison be chancre, and if recourse be not had to medicine to stop its progress; and even, in some cases, wherein the patient has availed himself of its use, the ulceration continues, extends, and destroys the whole penis, and only stops by the death of the individual. Where then are the confines of the “specific distance?” They are not to be found in the utmost boundary of the part!

If the disease be gonorrhœa, instead of being confined to one particular spot of the urethra, it often extends through the whole course of that canal.

Therefore, whatever may be the fact, as to any other specific disease being limited within certain distances, of which it would be impertinent to give any opinion here, we may conclude that in the venereal affliction, at least, the speculation is not warranted.

Happy would it be for many sufferers, if nature had prescribed limits to this terrible calamity. To their misery many have found that its effects have been as extensive as the system. These effects may continue local for a greater or less space of time according to contingencies. But

the disease, if unchecked, spreads itself at last, and the most distant parts feel the ravages of its presence. This, perhaps, is a circumstance of some moment; because a practitioner, who was really impressed with a belief that the venereal disease had its limits, might become more careless as to its progress, than in justice to his patient he ought to be.

We are, therefore, not to deceive ourselves, by thinking too slightly of this disease.—It is not bounded by nature.—It is not one of the most tractable and easy to manage.—The most anxious care, the most unremitted attention, are indispensably requisite.

CHAPTER VIII.

WHETHER THE EFFECTS PRODUCED BY THE POISON,
EXTERNALLY APPLIED, BE MADE WORSE BY
THE BLOOD BEING CONTAMINATED.

MR. HUNTER, in his section ‘ Of the
“ local and constitutional forms of the disease
“ never interfering with one another,’ observes,
“ To explain these effects more fully, let me
“ observe, that if a man has a gonorrhœa, and a
“ chancre appears some days after, the chancre
“ does not either increase or diminish the gonorr-
“ hœa. Again, if a man has either a gonorrhœa,
“ a chancre, or both, and a lues venerea ensue in
“ consequence of either of these, neither the go-
“ norrhœa nor chancre is affected by it. If a
“ man has a lues venerea and gets either a go-
“ norrhœa or chancre, or both, neither of them
“ affects the lues venerea, nor are their symptoms
“ the worse. Nor is the cure of either, singly,
“ retarded by the presence of the other; for a
“ gonorrhœa is as easily cured when there are
“ chancres, as when there are none, even although
“ the chancres are not attempted to be cured;
“ and a chancre may be cured locally independent

“ of the gonorrhœa. Further, a gonorrhœa,
 “ chancre, or both, may be as easily cured, when
 “ the constitution is poxed either by them, or
 “ previous to their appearance, as when the per-
 “ son is in perfect health; but the chancre has
 “ this advantage, that the constitution cannot be
 “ cured without its being likewise cured*.”

If there can be different degrees of virulence in a poison, and if the blood of a person in lues be contaminated, taking, I say, these two circumstances for granted, we might, I think, reasonably conclude, that the local effects of the venereal poison would be increased, or made worse, by the patient already having lues.

If, then, a person has his blood contaminated with venereal poison, and this person contracts a fresh chancre, is it not reasonable to suppose (if there be different degrees of disease), that it will be somewhat increased in virulence? And that the product will also be more poisonous? For, besides the cause which operates to the formation of poison in common cases, here is the concurrence of another, which is the contamination of that fluid from which the poison is formed; therefore, may we not prudently suspect a higher state of

* Hunter's Treatise, page 302.

disease and contamination of the matter of chancre thus circumstanced? But let us not further speculate.

All the cases so complicated, which have come under my care, I have found singularly obstinate, which confirms to me the truth of the reasoning; and, though one course of mercury may cure both complaints, this is no proof that the chancre or gonorrhœa was not of a worse nature, than it otherwise would have been.

CHAPTER IX.

WHETHER THE VENEREAL DISEASE BE THE CAUSE OF
OTHER DISEASES.

WHEN the different organs of the body perform justly their respective functions, we experience that pleasurable state called health; when any of those functions are interrupted or perverted, we experience new sensations and feelings, which are often painful, and this state we call disease. As health, then, is the natural consequence of a right performance of the offices of the several parts of the body, so is disease the necessary consequence of their improper action, which must have been occasioned by an adequate cause.

We allow that the human body is *subject* to, or *susceptible* of, disease. This quality of admitting a change, or variation, from the healthy action, has been denominated *disposition*, *tendency*, &c. As far, however, as these words are meant to signify any intention, in any part they
are

are incorrect: But though not by nature disposed or directed to disease, it is often forced, by circumstances, into that state.

The variety of diseases is very great, and their causes must also be numerous and diversified.

When disease occurs, it generally produces new sensations, new feelings, and new products.

Many of our opinions on diseases are, by no means, accurate. They are, in general, more allied to conjecture than to science. Much is yet to be done before our knowledge on this important point can obtain the merit of precision.

Among other questionable opinions, which have gained great circulation, may be classed the notion that one disease becomes the immediate cause of another.

Mr. Hunter has made some inquiry into this part of philosophy, and observes, "Every animal
" may be said to have natural tendencies to mor-
" bid actions, which may be considered as pre-
" disposing causes, and these may be called into
" action whenever the immediate cause takes
" place, which may be such as to have no con-
" nection with these tendencies, and cannot
" there-

“ therefore be considered as the cause of the dis-
“ ease. One disease excites another, and there-
“ fore is supposed to be the sole cause of it.
“ Thus slight fevers, or colds, small-pox, and
“ measles, become frequently the immediate cause
“ of scrofula; and certain derangements of the
“ natural actions of the body often bring on the
“ gout, agues, and other diseases; but these dis-
“ eases will be always more or less, according to
“ the constitution of the parts; and the constitu-
“ tions will differ according to circumstances,
“ which may be numerous; two of these, how-
“ ever, will be local situation, and age*.”

But we may ask, is not scrofula a family complaint? Probably it depends upon some organic or inherent cause in the body itself. How then can a fever, or a cold, become its *immediate cause*? Were this the case, should we not find the complaint much more prevalent than what it is? And in families not subject to this disease from the common source? But this does not seem to agree with experience. Mr. Hunter's opinions are entitled to a respectful consideration; but I cannot conceive how those diseases can become the immediate causes of scrofula at all.

* Hunter's Treatise, page 26.

The only way in which I can apprehend them to operate in advancing scrofula is, by making the body more susceptible of the action of the cause of this disease, be that cause what it may. Gouts and agues, also, have their respective causes. Certain derangements, and certain constitutions, may facilitate the operation of those causes; but this state of parts ought not to be confounded with the actual cause.

The venereal disease is also much accused of being the immediate cause of scrofula, and other complaints: I think the same fallacy here deceives. Venereal sores may become scrofulous, but this is no proof of venereal disease being the cause. The venereal poison will not produce scrofula; but, if this malady has altered the condition of parts, the cause of scrofula having the power to operate, we ought not, in my opinion, to say, that the venereal disease has immediately produced the other. The proof of this seems to be, that we never find venereal complaints become scrofulous, but in patients who have that complaint from a family taint,

Therefore, I cannot but be of opinion, that one disease seldom becomes the cause of others, and is only concerned in producing others,

others, so far as inducing a state of parts favourable to the operation of their causes. Of course, as far as I perceive the probability of the fact, I think that venereal disease seldom, if ever, becomes the actual, immediate cause of other diseases.

CHAPTER X.

PRACTICAL ADMONITIONS.

THE final view of our inquiries into disease, is to assist us in the cure. The more perfect our notions are, the more exact and effectual will be our practice. It is not for vain curiosity, we investigate the operations of the venereal poison.—We have another view, which is, individual advantage. That theory is serviceable, which conducts us to a rational and safe mode of cure; and that is an evil, which, when acted up to, diverts us from that practice which experience teaches us to adopt.

Some of the practical points of Mr. Hunter's doctrine are as follows (namely), that the venereal poison, when taken into the circulating fluid, becomes universally diffused; that it contaminates at once all the parts which are susceptible to its influence, and is then thrown out of the system along with some of the secretions; that the parts become diseased, some sooner, others later; and that,

in

in the intermediate time between the contamination, when the poison is first received into the blood, and the appearance of its visible effects, a new state is acquired, which he terms a *disposition*, which mercury will suspend, but cannot cure.

I have, in the last part of this work, inclined to the opinions, that the venereal poison, when received into the circulating fluid, may receive supplies; that this fluid affords a convertible principle, by which the poison is increased; and that it continues in that medium, and produces its effects, for an unlimited time, until expelled by the force of medicine. Whether it contaminates all that fluid, or certain portions of it only, I have not ventured to assert: It is an object highly worth attention.

Now, upon our conviction respecting these contrasted opinions, depends our mode of practice. If we incline to the opinions of Mr. Hunter, our practice will be to remove the visible and local effects by the use of mercury, then to discontinue it, and wait for fresh parts becoming diseased, which may have been already disposed. When the disease appears in those parts, to begin again the use of mercury upon the same principle; so on as often as the disease makes its appearance.

Or

Or, we should contract the first visible effects of the poison, and continue the mercury for life, to prevent the parts already disposed advancing to disease.

Now, if we incline to the opinions I have attempted to state, our intentions of cure will be, to cure the local effects of the poison, and remove the lurking and accumulating cause; or, in short, to continue the use of mercury for some time after the local effects of the poison are cured, with a view of preventing a relapse, or fresh parts becoming diseased.

As far as my observations extend, I think, if a venereal sore throat be cured by mercury, and that remedy be continued in proper quantities, and for a sufficient length of time after its cure, the disease willseldom relapse, or any other part become diseased: On the contrary, if that medicine be discontinued, as soon as the local injury is repaired, the disease often relapses, or fresh parts become diseased.

These circumstances incline me to be of opinion, that the poison is certainly increased in the blood, and they also seem to show to us, that the local effects of the poison are often cured before the poison in the blood is annihilated.

Depend-

Depending upon the advantage and necessity of such a practice, the following inferences seem suggested. That the action or mode of operation of mercury in the cure of lues, is two fold.

First, that by its effect upon the arterial system, the local effects of the poison are counteracted.— Secondly, that by its being received and mixed with the contaminated blood, it has the power of destroying the poisonous particles, or of suspending their accumulating power until they be thrown out of the system at its natural outlets.

To enter more fully into a practical discussion, would be irrelevant to the nature of this work. I leave the examination to an enlightened profession.

SUMMARY.



I will finish this book with a short, retrospective view of some of the principal opinions of Mr. Hunter, and those which I have endeavoured to vindicate.

Mr. Hunter's opinions are——

1st, That the two diseases, gonorrhœa and chancre, are effects of the same poison.

2dly, That the poison, in most cases, when applied to the human body, occasions an inflammation, and to this inflammation is joined a peculiar manner of acting, also produced by the poison; to which peculiar or specific mode of action, he attributes the formation of poison.

3dly, He afterwards disregards some of those assertions, and appears to be of opinion, that the poisonous quality of the matter is caused by an established law in the animal œconomy, whereby

whenever the irritable parts are caused to act by a peculiar substance or cause, so as to produce matter, that product necessarily possesses similar qualities to the cause which produced the action.

4thly, That when the effect of the poison is gonorrhœa, it ceases spontaneously; but not when it is chancre.

5thly, That the venereal disease is kept up by the "specific quality of the inflammation
"itself."

6thly, That the venereal poison, when absorbed by the lymphatics, and conveyed into the circulating fluid, by some means loses its specific principles, but still continues to be capable of producing disease.

7thly, That the local effects the poison produces, when in the blood, are all of one species, which is ulceration.

8thly, That the product of venereal sores is not poisonous.

9thly, That the secretions are not affected by the poison.

10thly,

10thly, That a pocky child cannot be produced either from the father or mother.

11thly, That when the venereal poison is received into the circulating fluid, it contaminates at the same time all the parts susceptible to its influence, and is soon thrown out of the system along with the secretions.

12thly, That the parts contaminated, do not immediately become diseased, but assume a state of disposition towards it.

13thly, That mercury can prevent and suspend the disposition, but cannot cure it.

14thly, That a part once cured, never becomes again diseased from the same infection.

15thly, That the effects of the venereal poison are limited within certain bounds.

16thly, That the venereal disease becomes the immediate cause of others.

Some of the principal conclusions I have espoused, and endeavoured to enforce, are as follow :

1st, That the poison of gonorrhœa and chancre is the same in the essential or poisonous principles.

Gonorrhœa and chancre seem to be of the same nature, and occasioned by the same cause, which produces different effects, according to the nature of the part to which it is applied.

2dly, That the poison, when externally applied, produces a kind of inflammation.

Whether applied to the urethra, or to a part covered with a more firm skin and cuticle, the inflammation often goes on to form matter.

3dly, That when applied to the urethra, a discharge of puriform matter flows from its orifice.

The peculiar character of the part, the nature of the disease, and the composition of the matter tend to prevent so frequent contamination in this state of the disease, as from chancre.

4thly, That the product elaborated by the inflammatory acting vessels, and from their untainted contents which they circulate, undergoes a change

change, and becomes poisonous, after leaving the small arteries which form it.

From observation, it would appear, that the product flowing from the inflammatory acting vessels, is thin and transparent, and afterwards becomes more thick and opaque.

5thly, That when the effect is chancre, the product is composed of the dissolved solid particles of the part with their juices, and a product of the inflammatory acting small arteries.

In this there is not a destruction, but a dissolution, decomposition or a change in the solid particles of the part while the chancre is forming and extending; these decomposed parts, together with their fluids and juices, and the product issuing from the broken arteries, compose the product.

6thly, That the matter issuing from these vessels also undergoes a change after leaving them, and becomes poisonous.

The inflammatory acting vessels, by their action upon their contents, and the assistance from the force a tergo, throw out a fluid, which at first
appears

appears thin and watery, and afterwards becomes thicker and more opaque.

7thly, That the venereal inflammation is produced, continued, and extended, by the poison.

The venereal complaint, no doubt, has many peculiarities, some of which are prominently marked. The singularities I conceive to be caused by the particular qualities of the poison, and that these characterizing effects, the *continuation* and *extension* are consequences of its poisonous influence.

8thly, That the effects the venereal poison produces, when taken into the blood, are venereal.

The venereal poison, when conveyed into the circulating blood, produces many local effects, which are venereal, of the same nature as those of chancre and gonorrhœa.

9thly, That the product of sores in lues venerea, is poisonous.

The consequences of the poison being externally applied to the vessels, often are inflammation and ulceration, and a poisonous product. When it is received into the circulating blood, and brought in contact

contact with the internal surfaces of the small arteries, the same effects often are produced. The product of these sores will again produce poisonous effects upon the same or different systems.

10thly, That the poison once received into the blood, continues to circulate in that vehicle an unlimited time.

The poison, when admitted into the circulating blood, remains in that fluid, and repeats its effects upon parts of the body, until destroyed or thrown out of the system through the natural outlets, and by the force of medicine; the powers of the body not being able to clear it of this pernicious cause.

11thly, That the poison increases in the blood.

We conclude, from the function of the arterial system, the numerous and copious excretions which it performs, the ingredients of which are taken from the common stock of blood it contains and circulates, that the poison would be soon got rid of, had it not the power of receiving supplies, or accumulating, when in that fluid.

12thly, That a part cured, may again become diseased from the same infection.

By the poison remaining and accumulating in the blood, it is enabled to produce effects upon different parts, which may be cured by the force of medicine; yet the poisonous cause, not being completely annihilated, recruits, and those effects are again repeated upon the same or different parts.

13thly, That the child in utero may be contaminated from the mother.

It would appear, that the child in the womb may be contaminated from the mother. I have inclined to dissent from the opinion, that it may be affected from the father.

14thly, That the secretions may be contaminated.

The blood from which the secretions are elaborated, being contaminated with the venereal poison, we conclude that the secretions themselves can hardly be quite wholesome.

15thly, That the product of sores in lues is venereal, from being formed out of tainted ingredients.

When a breach is formed in the solid parts, by the venereal poison being in the blood, a product
flows

flows from the broken artery, which is formed from the tainted blood which they circulate, hence becomes poisonous, as it leaves those vessels which make it.

16thly, That the effects of the venereal poison are not limited.

The effects of the poison, whether when applied externally or internally, if left to themselves, after a longer or shorter time spread, by which the sound adjacent parts are affected, and the malady only stops and declines by the use of medicine.

17thly, That the venereal disease seldom, if ever, becomes an immediate cause of other complaints.

When the venereal disease ceases, other complaints may commence, the body or part being left by that disease in a more susceptible state to the operation of the cause of the new malady.—This manner of forwarding disease ought to be carefully distinguished from an actual cause.

F I N I S.



1870
The first of the year was a very
cold one, and the weather was
very disagreeable. The snow
was very deep, and the wind
was very strong. The people
were very much distressed,
and the cattle were very
suffering.

The second of the year was a
very warm one, and the weather
was very pleasant. The snow
was very shallow, and the wind
was very light. The people
were very much pleased,
and the cattle were very
suffering.

The third of the year was a
very cold one, and the weather
was very disagreeable. The snow
was very deep, and the wind
was very strong. The people
were very much distressed,
and the cattle were very
suffering.

The fourth of the year was a
very warm one, and the weather
was very pleasant. The snow
was very shallow, and the wind
was very light. The people
were very much pleased,
and the cattle were very
suffering.

The fifth of the year was a
very cold one, and the weather
was very disagreeable. The snow
was very deep, and the wind
was very strong. The people
were very much distressed,
and the cattle were very
suffering.



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